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SURFACE MOTION OF WATER INDUCED BY WIND

By Dr. IRVING LANGMUIR

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ON August 7, 1927, when about 600 miles from New York on an Atlantic crossing to England I noticed that there were large quantities of floating seaweed, most of which was arranged in parallel lines with a somewhat irregular spacing ranging from 100 to 200 meters. These lines, parallel to the wind direction, which I shall call streaks, often had lengths as great as 500 m. Between these larger streaks, which contained vast quantities of seaweed forming continuous bands 2 to 6 m wide, there were smaller streaks which were made up of detached masses of seaweed along nearly straight lines. At this time the wind was from the north with a velocity of approximately 10 m/sec (22 miles/hr) and the waves roughly 4 m high.

A day later the waves were larger and the streaks of seaweed were still abundant. On the afternoon of this day a sudden change of wind direction occurred (of about 90°); within 20 min all the seaweed was arranged in new streaks parallel to the new wind direction, although the waves continued to move in the old direction.

It was clearly not cohesion between masses of seaweed that held them together in the streaks. At that time it seemed to me that the only reasonable hypothesis was that the seaweed accumulated in streaks because of transverse surface currents converging toward the streaks. The water in these converging currents descends under these streaks. Between the streaks rising currents, upon reaching the surface, flow out laterally toward the streaks.

The action of the wind on the water sets up longitudinal surface currents in the direction of the wind. The effect of the wind is thus to produce a series of alternating right and left helical vortices in the water

having horizontal axes parallel to the wind. If we face in the direction toward which the wind blows we should observe that the water between two adjacent streaks forms a pair of vortices: The water on the right-hand side of the vertical plane halfway between the streaks has a clockwise rotation (right helix), that on the left a counter clockwise rotation (left helix).

During the years 1928 and 1929 I made a large number of experiments on Lake George, near Bolton, New York to test this hypothesis of helical motion and to make observations on other water currents produced by wind.

This lake and the location chosen have many advantages for such studies. The depth is about 50 m and the water so clear that white objects at 8 m depth can be seen. Since the lake lies between two parallel mountain ridges, rising from 300 to 700 m above the lake surface, the wind usually dies down completely at night and during the day blows only in one of two directions (southwest or northeast). Most of the experiments were made at a point about 1 km from the nearest land with a clear fetch of from 4 to 7 km in the direction from which the wind was coming.

With wind velocities of 4 m/sec or more, streaks become visible because of the accumulation of traces of oil from motor boats, floating leaves or bubbles. Streaks are usually much more prominent in rivers contaminated by industrial oils or by organic matter from swamps.

A gram of oleic acid or olive oil applied to the surface of the lake spreads out to form a monolayer, covering about 1,000 square meters, and this area, because of the damping and elimination of capillary ripples, is sharply distinguishable from the surrounding uncontaminated surface by its smoother surface. After the oil has spread to its maximum area, however, it soon becomes invisible and no longer retards the formation of ripples. Oxidized lubricating oil after spreading to its maximum area leaves a still visible film which accumulates in the streaks and is thus more effective in rendering them visible.

In the autumn floating leaves are particularly useful to show the downward motion of the water under the streaks, for it can be seen from a motor boat brought over a streak that a large number of leaves are gradually carried down and disappear from sight under the streaks. The number of such descending leaves seems to be far too great to be accounted for by leaves which have gradually become denser than water. Some of the leaves placed on the water halfway between streaks reach the streaks, 6 to 10 m away, in about 5 min.

In November, 1929, further study of those motions was made by pouring 50 ml of a 2 per cent. fluorescein solution into the water from a bottle on the end of a bamboo pole. The motor boat was then moved away a suitable distance to prevent disturbance of the surface, and after definite time intervals the boat was brought over the place to observe the motion. It was found that when fluorescein was introduced at the center of a well-defined streak with a wind velocity of 6 m/sec it gradually moved downwards, taking 5 min to go 4 to 6 m. This corresponds to a vertical motion of about 1.6 cm/sec. The horizontal motion of the water on the surface was 15 cm/sec and 10 cm/sec at a depth of 3 m.

When fluorescein was similarly placed in the clear water between the two well-defined streaks it spread out irregularly over the surface and gradually moved toward the two neighboring streaks. The motion could not be followed as far as the streaks, as the fluorescein gradually became very diffuse, showing that although the motion was very slow it was very turbulent.

In other experiments a white cord 2 mm in diameter having small fragments of cork along its length at 10 cm intervals was floated on the surface of the lake in a straight line perpendicular to the wind direction. After 10 min this cord had developed well-defined waves, being displaced forward in the direction of the wind in the streaks and backward in the spaces between them, indicating that the forward velocity of the water was greater in the streaks than in the spaces between them. The water rising from deeper levels has a low forward velocity, but this increases steadily through the action of the wind so that when the water reaches the streak it has its maximum velocity.

A few measurements were made of the vertical components of motion by means of a large square sheet of aluminum suspended in a horizontal plane from a small lamp bulb so weighted as to give practically zero buoyancy. This device was calibrated in quiet water to determine the rate of descent or ascent when small weights were added or removed. This apparatus was lowered a few meters below the surface and attached by a light horizontal cord, 2 m long, to a lead weight suspended at an equal depth from a bulb floating on the surface. In this way the tendency of the aluminum plate to rise or fall could be observed without fearing the loss of the apparatus and without subjecting it to appreciable horizontal or vertical forces. In the streaks it was thus found that two meters below the surface there were descending currents of about 2 to 3 cm/sec and rising currents of from 1 to 1.5 cm/sec midway between adjacent streaks. This method appears particularly promising for future investigations.

To measure currents in the lake set up by wind, at depths from 5 to 30 m, some umbrellas, 60 cm diameter, were tilted 90° from their normal positions and suspended, with proper counter weights to hold them in this position, by light cords from small lamp bulbs floating on the surface. A painted bulb floating on the

surface attached to a lead weight resting on the bottom served as a marker buoy. By placing umbrellas at different depths close to the marker buoy and observing the motion of their floats the velocities and directions of the currents at different depths could be determined. The distances traversed by the floats were measured by the time required to cover the distance from the marker buoy to the float by a motor boat moving at a known speed.

In experiments on October 6, 1929, it was found that a "velocity indicator" at depths less than 5 m gradually drifted under a streak, but one suspended 10 m deep had no tendency to do so. Perhaps at a greater depth the indicator would move into a position midway between streaks, since there must be horizontal currents which converge under the rising currents between the streaks.

Between September, 1926, and August, 1929, on 28 separate days well distributed throughout the years, I measured the water temperatures at different depths (0 to 58 m) with an electric resistance thermometer.

When the lake freezes over, usually about January 10, the temperature at depths from 3 to 58 m is very uniform at about 1.2° C. By the end of March the temperature at the bottom rises to about 2.2°; but the increments are smaller at lesser depths.

This warming is due to the absorption of the sun's rays which penetrate the ice. The warmed water, being denser, sinks to the bottom and slowly flows to the deeper parts of the lake. Measurements have shown that in parts of the lake which have a depth of 5 to 10 m there is often a layer of warm water of 3 to 4° C. of 1 m thickness close to the bottom.

By the time the ice breaks up, about the middle of April, the temperature is again rather uniform, and is about 3° C., except for a thin layer of cold water under the ice. The rapidity of the breaking up and disappearance of the ice, which often takes only 10 to 20 hrs, is due to the warm underlying water stirred up by the wind as patches of open water are formed.

After the ice breaks up and while the water is within 1° or 2° of the temperature which gives maximum density (4° C.), the temperature on windy days is the same at all depths and rises at a rate of about 4° per month. At this time of the spring overturn, however, the temperatures on quiet days are especially nonuniform over different parts of the lake. In shallow places the temperature rises far more rapidly than in deep parts, for there are then no appreciable density differences to equalize the temperatures.

In 1926 the maximum temperature at 58 m was 9.6°, which occurred about November 10. In 1927 the temperature at 58 m depth rose at a gradually decreasing rate from 7° on June 1 to a maximum of 10° on November 15. During 1928 on June 1 the temperature at the bottom was only 5.3° and rose at a steady rate of only 0.7° per month until August 1. On October 20 the bottom temperature was 8.4°, which had been reached the previous year early in July. In 1929 a bottom temperature of 8.4° was reached as early as June 15, and it was 9.2 on July 6.

Examination of the temperature-distribution curves at different depths shows that between May and November a few quiet sunny days cause the development of a nearly uniform temperature gradient of as much as 0.7°/m which may extend to 10 or 15 m depth. A windy day causes the temperature gradient to disappear down to a certain depth, but produces a very sharp temperature gradient (as much as 5°/m) at the lower limit of the isothermal layer (thermocline). After alternating warm and windy periods the topmost 15 m of water may contain several of these isothermal layers (or epilimnions) with intervening thermoclines. The most marked gradients have always been observed at depths between 10 and 15 m.

I believe this represents the maximum depth to which the helical vortices descend with the wind velocities ordinarily occurring during the summer months.

When the surface temperature falls during the autumn the cooled water sinks to the level of water already of similar temperature. Thus the depth of the epilimnion increases during late September and October and reaches the bottom of the lake and produces the maximum bottom temperature about the middle of November. During this cooling the thermocline disappears, for the gradient below the bottom of the epilimnion is usually less than 0.3°/m.

Measurements with the velocity indicators have shown that longitudinal currents set up by wind extend to very different depths at different seasons. On June 20, 1929, at 9:00 A.M. when there was a 2 m thick, sharply defined, isothermal layer of water at 22°, overlying 16° water, a wind having an estimated velocity of 2 to 3 m/sec set the whole epilimnion into motion at 30 cm/sec. This lasted all day and continued for a couple of hours after the wind died down in the evening. The epilimnion was still 22° but had increased in depth to over 4 m.

Observations showed that after windless nights the water velocities at various depths were usually of the order of 2 to 3 cm/sec (rarely as high as 6) and had different directions at different depths. Within less than an hour after a wind of 4 to 8 m/sec springs up the water near the surface is set in motion parallel to the wind direction with velocities of 10 to 20 (rarely as high as 30), but the velocity and direction of the currents at depths greater than 10 m usually remains unchanged for at least 6 hours.

On July 27, 1929, the epilimnion (21.8°) had a depth of 6 m; the temperature gradient in the thermo-

cline was 1.0°/m to a depth of 15 m (12.5°). Below that, in the hypolimnion the gradient was only 0.07°/m. After a windless night a southwesterly breeze of 2 m/sec started about 8:00 A.M. (E.S.T.) and gradually increased to a velocity of 6 m/sec at noon, reached a maximum of 7m/sec at 2:00 P.M. and fell to 5 at 4:00 P.M. and to 1.5 at 7:00 P.M.

At a depth of 10 m the velocity during the whole afternoon was about 5 cm/sec in a direction 90° to the left of the wind direction, but at depths from 12 to 15 m the velocity was about 1 cm/sec in a direction which was 30° to the right of the wind. At depths less than 6 m the motion of the water was parallel to the wind. The momentum delivered to the water by the force of the wind must therefore be distributed within a layer 6 m deep.

At 12:30 P.M. the velocity of the surface was 20 cm/sec, and this rose to 24 at 3:20 and then increased slowly to 27 at 5:00 and decreased to 15 at 7:00 P.M. and was still 14 at 7:30 P.M. 20 min after the wind had died down completely. At a depth of 3 m the velocity was 14 at 4:00 P.M. and 10 cm at 7:30 P.M. At 6 m it was 9 at 4:30 and decreased to 7 at 6:00 P.M. and to 5 at 7:0 P.M. The velocities were also frequently measured at 1.3 m depth and were approximately halfway between those observed at depths of 0 and 3 m.

A rough estimate of the increase of momentum of the water which was caused by the wind was made by integrating $(v - v_0)dx$ from the surface down to a depth $x = 600$ cm, where the velocity remained approximately 6 during the day. The momentum/cm^2 of lake surface rose from about 6,000 (g. cm^{-1} sec^{-1}) at 4:00 P.M. to a maximum of about 7,000 at 5:00 P.M. and then slowly fell to 3,000 at 7:30 P.M. The maximum occurred about 3 hours after the maximum wind velocity and at a time when the wind velocity was only half its maximum velocity.

If we consider an infinite body of water the momentum per unit area due to the wind should be equal to $\int F dt$ where F is the horizontal force per unit area exerted by the wind. To estimate the order of magnitude of F we may assume that a momentum of 7,000 was delivered by wind acting for 3 hrs and so get $F = 0.65$ dynes/cm for a wind velocity of 5 m/sec (measured 2 m above the lake surface).

It is very evident from the measurements that the momentum does not increase steadily in proportion to $\int F dt$. There must then be some mechanism by which the momentum is transferred to the shores of the lake. A current of 10 cm/sec is only 0.36 km/hr, so that with a wind sweep of 5 km it should take 14 hrs for the effect of the shores to make themselves felt.

An unusual opportunity to study the effect of wind on the momentum of the water occurred on August 2, 1929, when there was a sudden reversal in the wind direction. At 9:00 A.M. on this day there was no wind and the surface water had a velocity of 2.9 cm/sec. By 10:00 A.M. there was a wind of 5.6 m/sec and the azimuth of the wind direction was 140°. The surface water was then moving 11 cm/sec (145°) and at 3 m depth the velocity was 6.3 (225°). At 12:20 the wind had increased to 8 m/sec, 120°, and at 2:50 P.M. had fallen to 3.5 m/sec, (90°), and the surface water was then moving 22 cm/sec in the same direction as the wind.

At 2:50 P.M. the wind suddenly reversed its direction. At 2:55 P.M. the wind velocity was 4, and the direction was 240°. The velocity decreased to 3.3 at 3:30 and rose to 7.0 at 3:45, the direction staying constant at 240°.

The surface water had a velocity of 13 at 3:12 P.M. and 18.5 at 3:35, the direction being 210° at both times. At 3:12 the velocity was 5.3 (185°) at 3 m depth and at 3:35 it was 3.2 (240°) at 6 m depth. Using these rather meager data, but assuming that the depth distribution curves were similar to those found on other occasions, I estimate that at 2:50 P.M. the momentum per cm^2 was about 6,000 in a direction at 90° and it was 4,000 (200°) at 3:12 and 6,500 (220°) at 3:35. This would mean a change of momentum of 8,300 in the first 22 min after the wind reversal and a further change of 3,000 during the next 23 min. A change of 8,300 in 22 min means $F = 6.3$ dynes/cm^2, a value 10 times as great as given by the data of July 27, although the wind velocity was lower than on that occasion. The rate of increase of momentum, however, rapidly decreased during the next 20 min.

On August 4 after 28 hrs of strong wind, 8 to 15 m/sec, of steady direction, the surface water had a velocity of 24 cm/sec, while the wind velocity ranged from 8 to 10 m/sec. This is a much lower velocity than was produced in June by a wind of only 2 to 3 m/sec.

On August 24 after 8 hrs of strong northeast wind the water velocities were 30 on the surface, 20 at 2.1 m and 19 cm/sec at 4.6 m depth, while the wind velocity was 11 m/sec 2 m above the surface.

Temperature measurements were made on the morning of August 25 after this storm had died down. The epilimnion (19.9°) had a depth of 10.1 m and the gradient in the thermocline was 1.4°/m to a depth of 14.6 m.

The velocities observed on September 2, 1929, with an isothermal layer (20.2°) of 11.5 m showed several interesting features. There was no wind until 9:30 A.M. and the wind velocity rose to 3.5 at 10:30, 5.0 at 11, 6.5 at noon, 9 at 1:00 P.M.; the velocity then decreased slowly to 5 at 3:00 P.M. Between 3:00 and 5:00 P.M. the wind was somewhat variable but aver-

aged 5 m/sec. It then decreased gradually to 3 at 6:00 P.M.

The velocity at 0.2 m depth was 11 cm/sec at 11:00, reached a maximum of 16 at noon, and decreased to 14 at 4:00 P.M. to 11 at 5:00 P.M. and to 4 at 6:00 P.M., although there was still a wind velocity of 3 m/sec.

At a depth of 1.3 m the velocity was only about 1.5 cm/sec less than at 20 cm. At 3 m depth the velocities decreased gradually from 10 cm at noon to 2 cm at 6:00 P.M. At 6 m the velocities ranged from 4 to 2.

The marked decrease in water velocity during the afternoon after 3:00 P.M. in spite of a nearly steady wind is in striking contrast to the observations earlier in the year in which currents were observed to continue with little decrease for hours after the wind died down. The falling off of velocities in September is undoubtedly due to the cooling of the isothermal layer by radiation into a clear afternoon sky which induces instability and causes the surface water to sink to the bottom of the isothermal layer, carrying its longitudinal momentum with it.

There is thus every reason to believe that the helical vortices set up by the wind extend to the depth of the epilimnion but do not penetrate through the thermocline. The surface of the lake is a free surface in the sense that there is no frictional force to restrain horizontal motion. The thermocline, however, is practically a fixed surface like that of a lake bottom, for it is not set in motion by the overlying layers. The longitudinal and transverse velocities of the water in the vortices have their maximum values at the surface and gradually decrease to zero at the thermocline. Thus the vortices are unsymmetrical in respect to depth, being increasingly diffuse at greater depths.

Observations of the streaks at different seasons show that in May and June, especially after quiet days when the epilimnion is shallow or is not strictly isothermal, the streaks are close together (5 to 10 m), while in October and November well-defined streaks usually have spacings of 15 to 25 m. The spacings are presumably approximately proportional to the depths to which they penetrate. Quantitative measurements of the streak spacings are difficult because between the well-defined streaks there are numerous smaller and less well-defined streaks. Just as large waves have smaller waves upon them, it appears that the surfaces of the larger vortices contain smaller and shallower vortices. The patterns of streaks on the lake surface are slowly changing; some growing, others dying out. On some days the streaks are much more regular than on others.

During the spring and fall overturns in April and early December when the whole lake is isothermal, the vortices may extend to the bottom of the lake, but they would then be very diffuse in their lower portions. On clear, cold windy nights in October the lower parts of the vortices should have their greatest velocities, since large-scale turbulence would then be stimulated by the descent of masses of denser water cooled by exposure on the surface at temperatures sufficiently above 4° C. to give a reasonably large coefficient of thermal expansion.

The helical vortices set up by wind apparently constitute the essential mechanism by which the epilimnion is produced. The currents thus set up at the bottom of the epilimnion may sweep off the upper part of the thermocline, making it thin and of increased gradient.

I have never observed in Lake George any reverse flow in the lower part of the epilimnion, but have frequently found an increase in the depth of the epilimnion at one end of the lake and a corresponding decrease at the other due to the wind. The return flow apparently usually takes place slowly at night and is not accompanied by the turbulence associated with the helical vortices and so does not give vertical velocity components which alone can give thermal transport to the deeper layers.

I have not made any search of the literature on this subject, but conversations with many students of turbulent flow and oceanography have indicated that the helical vortices induced by wind are not commonly recognized.

Professor C. Harold Berry has called my attention to a paper by James Thomson[1] in which he explains calm lines seen on a rippled sea" as the lines of convergence of surface currents. There is no suggestion, however, that the streaks seen with strong winds are caused in this way.

H. Jeffries[2] shows that there must be transverse circulation in streams with straight channels. He draws this conclusion from the fact that the greatest longitudinal velocity is observed at a certain depth below the surface near the middle of the stream. There are thus descending currents near the center and rising currents near the shores. Jeffries was not able to explain these transverse currents on the basis of hydrodynamical theory. Undoubtedly the mechanism is similar to that which causes helical vortices on the surfaces of lakes.

In 1933 I made numerous studies of the growth of waves under the influence of wind. I have found that the momentum carried by the waves and delivered to the shore as a radiation pressure accounts satisfactorily for the fact that the momentum of the water increases rapidly at first (before the waves have had time to build up) and then remains nearly constant. I expect to give an account of this work in another publication.

[1] *Phil. Mag.*, 4th series, 24: 247, 1862.
[2] *Proc. Camb. Phil. Soc.*, 25: 20, 1929.

SOME UNSOLVED PROBLEMS IN HUMAN ADJUSTMENT[1]

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ONE of the current fashions in psychology is to define its subject-matter as the organism in its environment, and the subject-matter of human psychology as the whole man in his whole environment. This orientation derives from many sources, prominent among them being an increasing knowledge of the close integration of all parts of the organism through neural and chemical mechanisms, and a growing recognition of environmental influences in the development of behavior. In this setting it is logical that one of the main themes of psychology should be the *adjustment* of the organism to its environment. It is the purpose of this paper to assemble for inspection certain peculiarities of so-called behavioral adjustment and to attempt to show their theoretical and practical significance.

The researches of Cannon,[2] Barcroft[3] and numerous other physiologists have demonstrated a remarkable capacity of the various organs and fluid media of the human body for establishing and maintaining a state of equilibrium or balance. They have likewise demonstrated the absolute necessity for the life of the organism of holding this balance within a slight margin of variation. Furthermore, they have gone far toward a complete explanation of the processes through which the equilibrium is maintained. This condition, called homeostasis by Cannon, is well illustrated in the case of the acidity-alkalinity balance of the blood, as reported by Cannon (p. 169).

> Blood has a slightly larger concentration of OH-ions than of H-ions, the index figure being approximately pH 7.4. Even minor variations from this reaction, which is just on the alkaline side of neutrality, are dangerous. If the hydrogen-ion concentration rises, so that the figure changes only to pH 6.95, coma and death result. And if the hydrogen-ion concentration falls, an increase in alkalinity as trifling as that indicated by a shift from pH 7.4 to pH 7.7 in the index figure, brings on tetanic convulsions. . . . In health, the variations from the normal reaction do not extend far enough beyond the close confines to impair the activities of the organism or to threaten its existence. Before such extremes are reached agencies are automatically called into service which act to bring back towards the normal position the disturbed state.

A similar need for establishing and maintaining a state of adjustment in the organism as a whole in relation to its external environment has been described by Raup.[4] He calls it the need for biological equilibrium and thinks of it as a state of metabolic balance. As analogous to the concept of Cannon, this biological equilibrium might be called organismic homeostasis. The maintenance of this balance or the return to it if it is disturbed is necessary for normal life. The mental reaction of the organism thus in equilibrium is a state of "good feeling" to which the author gives the name of "complacency." A condition of off-equilibrium, whose mental counterpart is loss of complacency constitutes a maladjustment. The return to complacency from a condition in which it is disturbed is the sole source of human satisfaction or pleasure. Inability to reestablish complacency means abnormality. Raup reduces all habit formation, learning, reflection and reasoning to a struggle to regain complacency, concomitant with the struggle of the organism to reestablish metabolic equilibrium.

For our present purpose there is no need to follow Raup into all the applications of his concept or indeed to go beyond his idea of a tendency toward a biological or metabolic equilibrium in the organism. The concept has been developed from the researches of Lillie, Jennings, Child, Herrick, Crile and many other biologists and physiologists. Indeed, when our knowledge of human physiology and psychology is complete, it may well be that organ homeostasis and organismic homeostasis will reduce to the same fundamentals. Thus an organismic reaction such as strenuous exercise with its excessive perspiration and consequent loss of body fluid may create an overconcentration of salts within certain of the body cells. There is set into action the machinery of the organs for reestablishing the balance (an organ reaction), which extracts fluid from the blood and lymph (the process of osmosis). This creates a water deficiency, whereupon the organism becomes thirsty. Its relation with its environment is thereupon disturbed (an organismic reaction), it seeks to allay its thirst, and doing so both restores the "biological equilibrium" and gives intense satisfaction. Thus, organismic reaction has led to organ unbalance, which in turn creates a disturbance of organismic equilibrium with the consequent struggle of the organism to restore it.

This instance will serve as a sample of one type of disturbance of organismic equilibrium with its reflection in a changed state of complacency. Most fre-

[1] Address of the vice-president and chairman of the section for Psychology of the American Association for the Advancement of Science, Indianapolis, December, 1937.

[2] Cannon, "The Wisdom of the Body," 1932.

[3] J. Barcroft, "The Architecture of Physiological Function," 1934.

[4] R. B. Raup, "Complacency: The Foundation of Behavior," 1925.

quently, however, it is the social disturbers of equilibrium reflected in changes in complacency which attract our attention. Our knowledge of the mechanism of these social disturbers is at present inadequate to enable us to associate them directly with the disturbers of organ equilibrium.

It is the purpose of this paper to present some evidence from psychological research concerning adjustment which does not readily lend itself to interpretation in terms of the struggle for equilibrium either organic or organismic. If the concept of balance or equilibrium were to be sufficiently extended to cover such cases, it might have to be conceived as a behavioral homeostasis, or perhaps even a "purposive homeostasis." It would, thereupon, become necessary either to demonstrate the positive correlation of these forms of homeostasis with biological or metabolic equilibrium, or to conceive of a state of complacency or mental equilibrium in the individual wherein it is in a state of organic disequilibrium or moving toward such disequilibrium.

Morgan,[5] working in the Columbia Laboratory of Psychology in 1916, attempted to measure the effects of noise upon efficiency of human performance. He reached what then appeared to be a surprising result, that not only was *output* of so-called mental work (in terms of which efficiency was measured) not always reduced but was frequently increased. It seemed as though the individual set for himself a certain level of performance, and in the struggle to maintain this level in the face of difficulties sometimes rose above it. This outcome, contrary to his hypothesis, led him to suspect that the work done under noise, although not reduced in quantity, might cost the organism more in effort per unit of work. The actual efficiency, measured in terms of output over cost would in such a case be reduced as he expected it would. Elaboration of his technique disclosed such evidence of increased effort in the form of changed breathing, talking aloud and increased tension in the muscular reactions. The experiment was further extended to the lifting of weights in which the energy expended could be objectively computed in terms of load, time and distance. Under appropriate instructions to keep the effort always at the maximum, it was found that in spite of instructions the effort expended varied with the load to be lifted, while the lifting time tended to be held constant.

> After one has been pulling a weight of 2,440 grams with what he supposes to be the maximum force he is able to exert, when unexpectedly a weight of 7,770 grams is substituted for the lighter one, his force at the very beginning of the pull is on the average 2.5 times as great as the supposedly maximum force previously used. The time taken for this adjustment ranges from .025 to .091 second with an average of .054 second. This is much shorter than the simple reaction time. . . . Since this adjustment is so rapid it can not be a conscious reaction. It must be a reflex or a local muscular adjustment.[6]

If any equilibrium or balance was being sought or maintained in this experiment it was not a metabolic balance, but consisted of a uniform rhythm or rate of movement. Morgan called this rate of work the "congenial pace." Here we seem to have an instance in which the metabolic equilibrium has, to a superficial analysis at least, been disturbed and remained disturbed when the experimental conditions were especially conducive for it to hold steady by the mere slowing of the rate of response. The state of complacency or satisfaction implied in Morgan's "congenial pace" is shown therefore to be associated with a state of off-equilibrium or increased expenditure of energy.

Davis,[7] using a more refined technique which involved the measurement of muscle action potentials, has confirmed the findings of Morgan. He has recorded the amount of increase of action potentials when distractions are introduced in the course of mental work. Meeting distractions does definitely increase muscular activity if action potentials are indicators of such activity.

Johnson,[8] in studying the sensitivity of the eye to differences in light intensity by means of reaction time technique, reports what he calls an incidental discovery. It is that under certain circumstances the more difficult the task of observing the light stimulus, the better is the quality of the performance as measured in terms of reaction time. He explains this discovery by saying that "the more unfavorable the conditions of observation the greater the degree of 'volitional attentive effort.'" This experiment furnishes an instance, then, of adjustment of attentive effort to maintain a given quality of reaction. That the adjustment overshoots the mark, so to speak, as it sometimes did in the experiments of Morgan, does not lessen the significance of the adjustment. There is, to be sure, no direct demonstration of increased energy expenditure in Johnson's subjects, but "increased volitional attentive effort" implies increased muscular tensions and these should have their counterpart in altered metabolic rate. Here, as in the study of Morgan, the individual does not get his satisfaction from maintaining a uniform level of energy expenditure but appears to sacrifice the maintenance of such a level in order to attain or maintain a standard of achievement.

[5] J. J. B. Morgan, *Archives of Psychology*, No. 35, 1916.

[6] J. J. B. Morgan, *Psychol. Rev.*, 27: 95–111, 1920.

[7] R. C. Davis, *Indiana Univ. Pub. Science Series*, No. 3, 1935.

[8] H. M. Johnson, *Jour. of Exp. Psychol.*, 1: 1–44, 1924.

The research of Thorndike and collaborators[9] on the effects of temperature and humidity upon mental efficiency offers similar data. Here the supposed obstruction to performance was such high temperature and high humidity as to make an extremely uncomfortable working milieu. As in the case of Morgan's use of noises, however, the output of mental work was not only not appreciably reduced, but in some cases was actually increased. There was a tendency to maintain uniformity of output, that is, to meet the disturbing conditions in some fashion. No measures of actual effort or energy expenditure were employed in this experiment so that any such changes would have to be inferred. Thorndike did, however, assume that an adjustment of some sort to the unfavorable conditions was being made and attempted to determine whether it was intentional or whether it was even known to the worker that he did so adjust. Manipulation of the experimental technique, so that it would not be possible for the worker to know either the quantity or quality of the work being done at any time gave the same evidence of adjustment. It seemed, therefore, that whatever adjustment was being made, whether by increased effort or otherwise, was not a consciously directed process. Could such a process of adjustment have biological equilibrium or complacency as its goal? Or was the goal some standard of achievement however set, some "congenial pace," which was sought regardless of cost to the organism?

The studies of Morgan, Johnson and Thorndike have been selected for report. Many others could be cited from nearly every realm of psychological research, showing a tendency to meet obstructions or difficulties by maintenance of pace in respect to quantity or quality or of rate of performance at the suspected or demonstrated cost of increased effort. In fact, the phenomenon is so universally present that Morgan in the study just quoted believes it to be a manifestation of a fundamental reflex present in all living tissue. It appears in its most rudimentary form, according to him, in the adaptation of the response of a single muscle to the resistance which it meets, where the greater the tension exerted upon the muscle the more strongly it contracts. This is a phenomenon noted many years ago by the physiologist Fick.

A peculiarly organismic or behavioral type of adjustment is demonstrated in the recent experimental studies of aspiration level as performed by Frank[10] and others.[11] In these instances there is no obstacle in the physical environment to be overcome, but the individual recognizes a certain level of adequacy in his performance.

> One thinks of himself as just so good. This is the "ego level" of the person. At the same time, there is an aspiration level—not the highest level imaginable, but the level which the organism sets as its goal and toward which it hopes it may possibly rise. The aspiration level is not set absurdly far beyond the ego level. The ego level is kept as high as it can be kept. The organism wants more than it conceives itself able to achieve. It constantly readjusts the two levels in the light of experience so that the ego level may be kept as close as possible to the aspiration level but yet never so close as to produce the shock of disappointment on sober recognition that the ego is not as good as one had hoped.

To use another terminology, of Sherif[12] particularly, one performs a task in a certain "frame of reference." It may be predominantly a personal or a social one, but whichever it is, it sets the level of aspirations. This level is just what its name implies, a goal toward which the individual aspires. He makes an effort to reach it. Failure to do so creates tensions and evidences of dissatisfaction.

One notes certain resemblances between the aspiration level of Frank and the "congenial pace" of Morgan. Both set a gait for achievement which the individual strives to reach and maintain. Morgan finds the tendency to maintain a pace in the face of obstacles a characteristic of all living organisms from the lowest to the highest. Frank finds it a characteristic of personality structure differing from person to person, but the relationship between aspiration level and past performance appears to be a "relatively permanent characteristic of the personality." Whether aspiration level will be reducible in the last analysis to an expression of the more fundamental congenial pace only further research will tell. Both, at any rate, imply setting a gait for achievement which the individual makes an effort to reach and maintain.

There grows out of this discussion a question of great theoretical and practical importance. Are these adjustments that are made in order to maintain some standard of performance in the presence of obstacles or to reach a higher level of achievement in accordance with level of aspiration costly to the organism? Or is this whole process one which goes on while the organism remains in a state of biological or metabolic equilibrium or approaches that state? For instance, does or does not work done in the midst of noisy surroundings or in atmospheric conditions of excessive heat and humidity take its toll of human energy? Do incentives of all sorts commonly employed to increase effort and thereby to increase efficiency really increase efficiency, or are they more costly when efficiency is properly

[9] E. L. Thorndike, See Report of the New York State Commission on Ventilation, Chap. 10, 1923.

[10] J. D. Frank, *Am. Jour. Psychol.*, 47: 285–293, 1935.

[11] G. Murphy, L. B. Murphy and T. M. Newcomb, "Experimental Social Psychology," pp. 212 *ff*., 1937.

[12] M. Sherif, "The Psychology of Social Norms," 1936.

computed? When the cost is measured in terms of effort, the researches quoted would seem to give a positive answer to these questions. The answer appears also to be positive when cost is measured directly in terms of muscle tensions or less directly in terms of action potentials of muscles. What is the answer when the efficiency of the organism as a whole is measured in terms of output over cost, the latter being expressed in terms of the energy expended by the organism in performing a unit of work?

An attempt has been made in our laboratory during the last several years to get a more direct answer to this question by measuring the metabolic cost of physical and mental work under a great variety of conditions, such as quiet and noise and with different incentives to increased performance. The work is still in progress and the question has not received a final answer, yet there are certain indications of what that answer will be. We[13,14] have studied the metabolic cost of doing mental work such as adding columns of figures in a quiet room, in a room where the noises from a large number of typewriters were reproduced phonographically, and where the noises of a busy city corner were similarly reproduced. When the noises were introduced there was a temporary reduction in output of work accompanied by an increased metabolism per unit of work done. This is according to expectation derived from the studies of Morgan. But if the noise is continued, the worker in a surprisingly short time returns to his normal output and there is a corresponding return of the metabolic rate toward the normal. The data are highly variable, the number of subjects upon whom they have been obtained is few, and the duration of the experiments which we have thus far been able to complete has been too short to justify any very positive statements at the moment. But it does seem as though the organism has some way of meeting such obstacles to performance as were employed in our experiments so that after a kind of adjustment period output may be maintained at its normal pace without the expected increase in energy cost per unit of work. Just how long the organism could thus continue without loss in efficiency after the adjustment had occurred or whether such loss would ever occur is a problem in itself.

An interesting bit of corroborative evidence for these metabolism findings comes from the measurement of muscle action potentials. Davis[15] measured the changes in tension which certain voluntary muscles undergo when an individual is subjected to a noise distraction over a period of time. On the first day of the experiment the magnitude of the action potentials was high, but even within a single sitting there was a noticeable decrease. This persisted until the following day, and in addition there was a cumulative adaptation over a series of days, so that at the end of five daily experiments relatively little tension remained.

The data from our experiments on the effects of incentives[16] are still less final, but the trend of the indications is in the same direction. The mental work consisted in adding columns of two place numbers and the incentive was a money bonus ranging from a few cents to five dollars for exceeding one's previous record in a spell of work. Incentives do seem capable in some instances of increasing output with no increase in metabolic cost per unit of work and at times even with a lessened cost per unit of work.

We have come at last to the unsolved problems to which the title of this paper refers. How are such disturbers as noises, lights, pains, heat and humidity met and dealt with so that effort may be exerted without increasing energy expenditure or so that neither increased effort nor increased energy expenditure need occur. Shall we at this point be forced to take leave of a purely mechanistic interpretation of behavior wherein an increase of load calls for an increase of power? Shall we be forced to rest the burden of explanation upon the exercise of the will! Let us look for alternative suggestions, even though support for them may be tenuous.

Introspective reports taken during the distraction studies may throw a little light upon our problems. Distractions have generally been defined in objective terms. A noise is produced while one is working at arithmetic problems, lights are flashed while one is listening to words to be memorized, high atmospheric temperatures are introduced while one is judging the quality of compositions. Are they distractions? Strictly speaking, they are distractions or distractors only if they distract. Many of the introspective reports show that when the objective conditions for distraction are present, the subject is not distracted. He may say that he "paid no attention," he "was not bothered" or he did not "notice the supposed disturbers." In such cases no increased effort seemed to be necessary and the output, as would be expected, was normal.

To account for the fact that distractors sometimes do not decrease output of work and may even increase it, by saying that distractions do not distract is merely raising another question which demands an answer. Why do environmental conditions that are called distracting not distract? How can the organism "not be bothered" or just not pay attention? One suggestive bit of evidence to meet these questions comes from the research of Davis just described in which muscle action potentials decreased during a five-day distrac-

[13] F. L. Harmon, *Archives of Psychology*, No. 147, 1933.
[14] Rounds and Poffenberger (unpublished).
[15] *Op. cit.*

[16] Rounds and Poffenberger (unpublished).

tion experiment. Whether such changes in muscle tension are causes or consequences of the adaptation can not be stated definitely at this time. But if the former can be shown to be the case, then it would be a good guess that the peripheral sensory mechanisms together with the voluntary musculature taking part in the sensory adjustment process performed a protective function against distractors. By a reduction in tension or relaxation they would transmit the otherwise disturbing stimuli at a reduced level of intensity, a level too low for effective competition with more favored events.

If the tentative explanation just given seems to be weak or faulty, another is available that is almost exactly opposite in its implications. It may be introduced by the observation that not all conditions of distraction are resolved by ignoring them, or by not being bothered. The effort clearly demonstrated by Morgan is the response more commonly noted, particularly at the onset of a disturbance. Thereby the subject seems somehow to keep his performance to a prescribed level, or in attempting to do so to exceed it.

What is the utility of the added effort, demonstrated by Morgan and inferred by both Johnson and Thorndike, in maintaining a level of achievement or in raising that level? Is such effort merely the subjective experience of wasted energy? The answer is not immediately obvious from a casual examination of the accessory mechanisms that are thus brought into action. It is the old physiological and psychological problem of dynamogenesis, but some light is being thrown upon it by current researches. The work of Freeman,[17] who measures the specific tension patterns in groups of muscles by means of mechanical levers, and that of Jacobson[18] and Davis,[19] who measure muscle tensions in terms of action currents, establish without question the existence of the tensions during effort. The work of Bills[20] and of Block,[21] who deliberately induce muscle tensions in the course of mental work and measure their effects upon performance, gives quantitative evidence of the reinforcement that may thus be produced. And the work of Hartmann[22,23] and others in their demonstration of the increase of subjective intensity of certain sensory experiences as a result of the simultaneous stimulation of other sense organs gives a hint of a possible mechanism of reinforcement in the presence of sensory distractors. If the stimulation of *other* sensory mechanism raises the subjective intensity level of those experiences mediated by the sense having dominance at the moment, the latter should thereby become the more effective. In certain favored circumstances, therefore, the expected consequence of so-called distractors should be an increase in the efficiency of behavior. Are these favored circumstances to be found in the states of muscular tension such as those whose effectiveness has been demonstrated by Bills?

Two types of explanation for the meeting of distractions have now been offered: One was derived from introspective reports of certain subjects who were not distracted and from the study of the process of adaptation to distractions showing a reduction of muscle tensions with time; and the other derived from the study, particularly, of that period immediately following the onset of a distraction and showing a reinforcement by way of the activation of accessory mechanisms. These two views, seemingly antagonistic, may be tentatively reconciled in either one of two ways.

The whole complex process of reacting to competing stimuli may be conceived as a process of sensory conditioning. Cason,[24] after determining the subjective intensity of certain visual and auditory stimuli when presented separately, gave his subjects what he called a conditioned response training. This consisted in "evoking simultaneous visual and auditory responses a large number of times." Before the training, a stimulus affecting either one of two senses had the effect of increasing the subjective intensity of the other, thus supporting the findings of Hartmann mentioned above. "But after the conditioned response training, a stimulus affecting one of the two senses had the effect of decreasing the intensity of the other simultaneous sensory response." Thus, our two supposedly conflicting interpretations would seem merely to cover the two stages of an adjustment process, the first a reinforcing and the second a weakening one. There would remain to be explained just why in the first case the "appropriate" stimulus would be reinforced and why in the second case the distracting stimulus would be weakened. However, one need only appeal to the concept of "dominance" as elaborated by Razran[25] to account for such selective action in the phenomena of conditioning.

Or, one may accept the increased tensions noted by Morgan as authentic and interpret the reduced tensions demonstrated by Davis as a *shift of tensions* to new muscle patterns with a reduction of tension only in those muscles on which measurements were at the time being made. The decreasing metabolic cost as adaptation to the distraction progresses would indicate merely that the later tension patterns were less

[17] G. L. Freeman, *Jour. Gen. Psychol.*, 5: 479 *ff.*, 1931.
[18] E. Jacobson, *Am. Jour. Psychol.*, 44: 677 *ff.*, 1932.
[19] R. C. Davis, *Indiana Univ. Pub. Science Series*, No. 5, 1937.
[20] A. G. Bills, *Am. Jour. Psychol.*, 38: 227 *ff.*, 1927.
[21] H. Block, *Archives of Psychology*, No. 202, 1936.
[22] G. W. Hartmann, *Jour. Exp. Psychol.*, 16: 383–392, 1933.
[23] G. W. Hartmann, *Jour. Exp. Psychol.*, 17: 813–822, 1934.
[24] H. Cason, *Jour. Exp. Psychol.*, 19: 572–591, 1936.
[25] H. S. Razran, *Psychol. Rev.*, 37: 25–43, 1930.

extensive or less complicated patterns or functioned more economically than the earlier ones. The tension patterns would shift but would never disappear. There is evidence for such shifts of pattern in the experiments of Freeman[26] and of Jacobson.[27] And the data of Jacobson give some support for the view that a tension pattern is always present during mental activity, since such activity is reduced to a minimum during a state of muscular relaxation.

Our possible explanations of the economy of adjustment are still not exhausted. Let me merely suggest a third, namely, the *law of least action* as propounded by Wheeler.[28] Any situation which presents an obstacle creates a condition of strain or tension. And any effort to overcome strain is an activity directed in a line of least action. Wherever there are two or more potentials belonging to a particular energy system motion takes place from the higher to the lower stress until an equilibrium is established. An illustration employed by him is particularly pertinent at this point. I quote freely: A lecturer and an indifferent audience.

> Why was the audience indifferent? But, first, what is indifference? It is a strain. The audience was bored; the seats were hard; the air was stuffy. The people preferred other activities than listening to an uninteresting speech. Accordingly, movements and attitudes on the part of the audience were not only symptoms of this strain, but they were also efforts to compensate for it. Had it not been inhibited by fear of breaking a long established custom and thus subjecting itself to criticism, the audience would have walked out to relieve itself of this strain, but courtesy to the lecturer prompted it to remain in the hall and to make the best of it.

They obeyed Wheeler's law of least action!

I hope that the several tentative explanations that have just been proposed have not served merely to belittle the remarkable fact that the human organism can adjust itself to changing conditions with an economy that man-made mechanical devices can not imitate; or to give the lie to the title of this paper which says that these problems are still unsolved. They are unsolved but not insoluble. The unknowns are gradually being whittled away. In the meantime, let us remind ourselves that organismic homeostasis is not inactivity, that complacency is not laziness, that the law of least action does not imply a saving of one's energy, and finally that having aspirations and striving to attain them are normal human organismic reactions deeply rooted in mechanisms as fundamental as the reflex.

[26] *Op. cit.*
[27] *Op. cit.*
[28] R. H. Wheeler, "The Science of Psychology," Chap. 3, 1929.

OBITUARY

JOHN KUNKEL SMALL

Dr. John Kunkel Small, chief research associate of the New York Botanical Garden, died at his home in Bronx Borough, New York City, on January 20, 1938, in his sixty-ninth year. He was of German descent, and was born in Harrisburg, Pennsylvania, on January 31, 1869. At 19 he entered Franklin and Marshall College at Lancaster, Pennsylvania. His first botanical paper was published during his sophomore year, and six more followed before his graduation in 1892. Of these early contributions to botanical science, two were concerned with mosses, and two were prepared in collaboration with his classmate, A. A. Heller, well known for his later work in the botanical field.

A few months after his graduation from Franklin and Marshall, he entered Columbia College, now Columbia University, with a fellowship in botany, and there he remained for six years, studying and serving as curator of the herbarium. His attention then, and throughout the years that followed, was concentrated upon the taxonomy of flowering plants and the flora of the southern United States.

Upon his arrival in New York he commenced work at once upon the genus Polygonum, and published five preliminary papers upon this subject. When he received his degree as a doctor of philosophy in 1895, his volume, "A monograph of the North American species of the genus Polygonum," was the most sumptuous American botanical thesis ever published. Meanwhile he had studied various other plants in his customary critical way, and had begun work on the Oxalidaceae, with which his name has since been closely associated.

His interest in the southern flora began at least as early as the summer of 1891, when in company with Heller he made a trip to western North Carolina, and continued unabated throughout his life. During his six years at Columbia he prepared a series of fourteen "Studies in the botany of the southeastern United States"; these comprised more than a hundred pages, and were published, as were most of his botanical writings of this period, in the *Bulletin* of the Torrey Botanical Club.

He had joined the Torrey Club in January, 1890, two and a half years before he made his home in New York, and always identified himself with this, the oldest American botanical society; he was elected an honorary life member in 1934. He became a member

of the American Association for the Advancement of Science in 1895, and a fellow in 1902. But he persistently held aloof from entanglements with other scientific societies.

When the staff of the New York Botanical Garden was organized, in 1898, he was one of its original members, with the title, curator of the museums. In 1906 this was changed to head curator of the museums, and so continued until the end of 1932; he was then relieved of his administrative duties, in order that he might devote his time more completely to the research upon which he had been engaged so assiduously throughout his career.

His transfer from Columbia to the garden was not marked by any important change in his activities. He had been in charge of the Columbia herbarium, and he continued in charge of the herbarium of the garden with which that of Columbia was combined a few years later. He had been devoting as much time as he could to study of the southern flora, and this he worked upon with unabated enthusiasm. In 1903 was published his monumental work, the "Flora of the Southeastern United States," a bulky volume of nearly fourteen hundred pages; a new edition of this flora appeared in 1913; and his work in this field culminated in 1933 with the appearance of his "Manual of the Southeastern Flora," with figures illustrating every genus.

In the course of his earlier studies on the flora of the southern United States he became more and more impressed with the importance of Florida in the general scheme. The flora of Florida was varied by the southern extension of northern forms and the northern extension of tropical ones; no systematic botanical exploration of the state as a whole had ever been attempted, and it was apparent that here was the most promising field for such exploration in the entire country. He first visited Florida in 1901, and during the succeeding thirty-five years he returned more than thirty-five times, throwing himself into the work of exploration with unquenchable zeal. When circumstances finally put an end to his trips, he possessed unparalleled first-hand knowledge of the flora of all parts of the state. In the course of these trips he formed many friendships with kindred spirits, and such friends will join his closer associates in mourning his loss. Some of his later southern trips extended westward along the Gulf coast as far as Texas.

In most of his books he discussed flowering plants only. This was because he felt that the word "flora" belonged specifically to "flowers" and was inappropriate in the case of ferns, which are included in most so-called "floras." It was because of this view that he planned certain smaller works devoted exclusively to ferns and fern-allies. His first fern paper was published as early as 1890, and was followed by others at rather infrequent intervals. In 1918 he issued two small books on the ferns of southern Florida; in 1932 a larger one entitled "Ferns of Florida"; and in 1935 a still larger work, "Ferns of the Vicinity of New York." He became increasingly interested in this group of plants, and spent much of the time during the past year revising the proofs of his "Ferns of the Southeastern States," a book of 500 pages, with a full-page illustration of each species, which will make its appearance in the near future.

When the late Thomas A. Edison, realizing that war might at any time deprive this country of its sources of supply of rubber, began his series of experiments with native plants and such as could be grown on a commercial scale within our limits, as a possible source of this product, it was to Small that he turned for advice on the botanical aspects of his study.

In his later years he was saddened by the changes wrought in his beloved Florida by the advances of so-called "civilization." One after another he saw the most interesting plant communities and the most beautiful scenery in the state perish by the hand of man, until it was sometimes difficult to determine exactly where they had been. Then it was that he wielded his pen with telling effect in defense of Florida's remaining wonderlands.

In 1912, when his alma mater, Franklin and Marshall College, celebrated the 125th anniversary of her establishment, she conferred upon him the honorary degree of doctor of science. In 1936 he received the award of an honorary scroll from the Alumni Association of the Graduate Schools of Columbia University for his contributions to science as botanist, explorer and author.

Little remains to be said of his life work. He devoted himself to the advancement of botanical science with unflagging energy. Blessed with a rugged constitution, he never spared himself in his work, whether wading all day in southern swamps or writing in his study long after midnight. Perhaps he shortened his days by his overexertion; but, at all events, his was a full life—full of enjoyment, full of accomplishment. His life was concentrated in his work. His only recreation was furnished by his lifelong interest in music, which he transmitted to his children, but this aspect of his career needs only passing mention here.

His scientific writings were characterized by originality, honesty and fearlessness. His view-point was always that of a progressive, and was not always appreciated by his fellow-workers. But the writer of these words, who was his friend for forty-four years,

believes that when the conservatism that is still rampant has broken down, the name of John K. Small will be ever more highly revered.

J. H. BARNHART

LEO D. WHITNEY

FOLLOWING an illness of but a few days, Leo D. Whitney, assistant agronomist of the University of Hawaii, died on November 7, 1937. Born in Willits, California, on May 11, 1908, he secured his early education in the schools of Santa Rosa, and received his B.S. degree from the University of California in 1933, having majored in agronomy. The next two years were devoted mainly to graduate study in botany. Before completing his work for the Ph.D., however, he was called to the University of Hawaii to undertake special studies on grasses and on taro. He made very valuable contributions to the taxonomy of Hawaiian grasses, listing a number of new species, not before described, from Hawaii. His work dealt especially with economic pasture grasses. He had, at the time of his death, prepared for publication a list of 150 grasses, established on Hawaiian ranges. One of his principal tasks at the University of Hawaii was to attempt to work out a key for the classification of Hawaiian taro varieties. Out of more than 200 so-called varieties, he was able to describe 85 distinct horticultural varieties and to develop a satisfactory key for their identification. He also made a very distinct contribution to the knowledge of Aroids in general through his work on the seeding and mutations of taro. He was a member of the American Association for the Advancement of Science, Botanical Society of America, Hawaiian Botanical Society and a number of other botanical and taxonomic societies.

Mr. Whitney was a conscientious worker, thorough student and sincere friend. His untimely death came as a severe shock to his many friends and associates, and removed prematurely one of the most promising young agronomists of the present generation.

B. A. MADSON

RECENT DEATHS

DR. HARRY W. TYLER, professor emeritus of mathematics of the Massachusetts Institute of Technology, secretary of the American Association of University Professors from 1916 to 1933, died on February 2 in his seventy-fourth year.

DR. HOWARD E. SIMPSON, head of the department of geology of the University of North Dakota and state geologist, died on January 31. He was sixty-three years old.

SIR JAMES CRICHTON-BROWNE, London, specialist in mental and nervous disorders, died on January 31 at the age of ninety-seven years.

SIR AMBROSE THOMAS STANTON, chief medical adviser to the British Secretary of State for the Colonies, adviser to the government on tropical diseases, died on January 25 at the age of sixty-two years. Sir Thomas had been bacteriologist at the Institute of Medical Research, Kuala Lumpur, Federated Malay States, becoming director of the institute in 1920.

WILFRED HENRY PARKER, for eighteen years director of the British National Institute of Agricultural Botany, died on January 11 at the age of forty-nine years.

THE death is announced of Dr. A. B. Martynov on January 29. Dr. Martynov was an entomologist, known for his investigations on insect paleontology and evolution. He has been stationed during the last year or so at the Institute of Paleozoology at the Academy of Sciences in Moscow.

SCIENTIFIC EVENTS

RESEARCH ON CHRONIC DISEASES AT WELFARE ISLAND

DR. S. S. GOLDWATER, commissioner of the Department of Hospitals of New York City, has announced the receipt of an appropriation of $66,000 from the Rockefeller Foundation for the support of research on chronic diseases to be conducted on Welfare Island. This work was inaugurated two years ago and is conducted by a research staff appointed by the Department of Hospitals and supported in part by the City of New York. Supplementary funds required for the conduct of intensive research work have heretofore been obtained from various sources by the Research Council of the Department of Hospitals, to which the Rockefeller grant of $66,000 has been intrusted for expenditure over a three-year period.

The officers of the Research Council of the Department of Hospitals are: Marshall Field, *president;* Dr. John A. Hartwell, *vice-president;* Dr. Bernard Sachs, *treasurer,* and Dr. S. S. Goldwater, *secretary.* The Research Council acts on the advice of a scientific advisory group, under the chairmanship of Dr. Alfred E. Cohn, of the Rockefeller Institute.

On the completion of the Welfare Hospital for Chronic Diseases the work of the research division, which is now being carried on in temporary quarters at Metropolitan Hospital, will be transferred to the new 1,600-bed Welfare Hospital, now rapidly approaching completion.

The following is a résumé of the scientific work now in progress:

Dr. Forrest E. Kendall has been studying the immunological behavior of the proteins of human sera with particular reference to a number of chronic diseases. Dr. Henry W. Dawson and Dr. Ralph Boots, together with Dr. Kendall, have been studying rheumatoid arthritis. A reprint of Dr. Kendall's work appeared in the *Journal of Clinical Investigation*.

Dr. Arthur J. Patek, Jr., has initiated studies on the rôle of nutrition in chronic disease of adults. Cirrhosis of the liver has been selected for special investigation. Estimation has been made of the nutritional state of patients and of their response to high vitamin diets.

In addition, Dr. Patek has carried on studies on the factors causing abnormal coagulation of the blood in hemophilia. Dr. Patek's first report of the work has appeared in the *Proceedings* of the Society for Experimental Biology and Medicine.

Dr. Kenneth B. Turner and Dr. Alfred Steiner are studying the cholesterol metabolism in an effort to determine its rôle in arteriosclerosis. They have had the opportunity of determining the normal variation in the level of the blood cholesterol over a long period. They have further studied the effect on cholesterol level of such factors as cholesterol ingestion and the administration of potassium iodide and thyroid extract. The relation of hemolytic streptococcus infection to the production of atherosclerosis is being investigated. The action of choline on the formation of lipoid deposition in the arteries is under study.

Dr. Dickinson W. Richards, Dr. Andre Cournand and Dr. James Scott Mansfield are occupied with physiological studies in respiration. An investigation is being made of circulatory and respiratory functions in pulmonary emphysema with particular reference to the functional residual air. Dr. Mansfield is conducting an investigation of the hypertension produced by renal ischemia. Dr. Mansfield and Dr. David Weeks are studying the possible effect on the hypertension of increasing collateral circulation to the kidneys.

Dr. Giles W. Thomas is conducting psycho-somatic studies in rheumatic arthritis.

Dr. David Seegal is working on bacteriological and immunological factors in acute and chronic glomerulonephritis.

THE SIGMA XI LECTURE SERIES AT YALE UNIVERSITY

THE second series of the Yale Sigma Xi Lectures, which was devoted to "The Chemistry of Substances Important in Living Organisms," was opened on January 19 with a lecture by Professor C. N. H. Long, head of the department of physiological chemistry, who spoke on "The Hormones and Their Relation to the Processes of Metabolism." Since considerable progress has been made in recent years in the identification of the chemical nature of the hormones he also gave a brief review of this work along with an indication of their structural relationships to other substances.

The second lecture will be given in February by Professor R. J. Anderson, who will discuss "Chemical Studies on the Tubercle Bacillus and other Acid-fast Bacilli." For the past ten years Professor Anderson has been directing a cooperative research project in the Sterling Chemistry Laboratory which has as its purpose the isolation of the chemical substances of these bacilli.

The April lecture, to be given by Professor Northrop, will be devoted to the "Chemical Nature and Mode of Formation of Pepsin, Trypsin and Bacteriophage." This lecture is of special interest to the investigators at Yale, since one of the pioneer workers on bacteriophage, Félix d'Herelle, carried on his research in the laboratories of Yale University.

The concluding lecture will be given by Professor Cowgill, who will speak on "Vitamins and Other Important Nutritive Factors." He will give a discussion of the substances which have been shown to be necessary for proper nutrition of the living organism, their chemical nature and their functions in the body as essentials to health.

The subject of the first series of lectures was "Modern Physics: Nuclear and Interatomic Reactions." The lecturers and their subjects were as follows: Professor A. F. Kovarik, "Survey of the Field, Methods of Attack and Nuclear Transformations"; Professor E. Pollard, "The Nuclear Energy Levels and Nuclear Dimensions"; Professor W. W. Watson, "The Spectroscopic Study of the Atomic Nuclei"; Professor H. Margenau, "The Forces within Atomic Nuclei"; Professor L. W. McKeehan, "Magnetization in Crystals: Ferromagnetic," and Professor C. T. Lane, "Magnetization in Crystals: Paramagnetic." The Sigma Xi lectures are open to the public and are given on Wednesday evenings.

OFFICERS OF THE WASHINGTON ACADEMY OF SCIENCES

AT the fortieth annual meeting of the Washington Academy of Sciences on January 20, 1938, the results of the election of officers for 1938 were announced as follows: *President,* Paul E. Howe, Bureau of Animal Industry; *Non-resident Vice-presidents,* James Franck, the Johns Hopkins University, and W. T. Thom, Princeton University; *Corresponding Secretary,* Nathan R. Smith, Bureau of Plant Industry; *Recording Secretary,* Oscar S. Adams, Coast and Geodetic Survey; *Treasurer,* H. G. Avers, Coast and Geodetic Survey; and *Members of the Board of Managers* for three years, F. G. Cottrell, Research Associates, Inc., and Neil M. Judd, National Museum. *Resident Vice-presidents,* nominated by the affiliated societies, were elected as follows:

Philosophical Society—N. H. Heck
Anthropological Society—Henry B. Collins
Biological Society—H. C. Fuller
Chemical Society—F. C. Kracek
Entomological Society—C. F. W. Muesebeck
National Geographic Society—A. Wetmore
Medical Society—Fred O. Coe
Historical Society—Allen C. Clark
Botanical Society—W. A. Dayton
Archeological Society—Aleš Hrdlička
Foresters—G. F. Gravatt
Washington Engineers—Paul C. Whitney
Electrical Engineers—H. L. Curtis
Helminthological Society—E. W. Price
Bacteriological Society—L. A. Rogers
Military Engineers—C. H. Birdseye
Radio Engineers—H. G. Dorsey

AWARDS OF THE AMERICAN SOCIETY OF CIVIL ENGINEERS

DR. HENRY EARLE RIGGS, honorary professor of civil engineering at the University of Michigan, took office as president of the eighty-fifth annual meeting held in New York from January 19 to 22 of the American Society of Civil Engineers.

Winners of medals and prizes for contributions to engineering science, awarded annually by the society, are:

J. C. Stevens, consulting hydraulic engineer, Portland, Ore.—The Norman Medal for a paper on "The Silt Problem," adjudged the outstanding contribution of the year to engineering science.

Inge M. Lyse, research professor of engineering materials, in charge of the Fritz Laboratory, Lehigh University, and Bruce G. Johnston, instructor, civil engineering, Columbia University, jointly—The J. James R. Croes Medal, for their paper on "Structural Beams in Torsion," considered next in order of merit to that contributed by Mr. Stevens.

Eugene A. Hardin, assistant civil engineer, Department of Public Works, Detroit—The Thomas Fitch Rowland Prize, for a paper on "The Springwells Filtration Plant, Detroit," considered the best paper describing in detail "accomplished works of construction, their cost and errors in design and execution."

Boris A. Bakhmeteff, professor of civil engineering, and Arthur E. Matzke, both of the department of civil engineering, Columbia University, jointly—The James Laurie Prize, for a paper on "The Hydraulic Pump in Terms of Dynamic Similarity," considered next in order of merit to that submitted by Mr. Hardin.

E. C. Harwood, Captain, Corps of Engineers, U.S.A. (retired), Cambridge, Mass., director of the American Institute for Economic Research—The Arthur M. Wellington Prize, for a paper entitled "Proposed Improvement of the Cape Cod Canal," for the best paper on transportation "on land, on the water, or in the air."

Victor L. Streeter, assistant engineer, U. S. Bureau of Reclamation, Denver—The Collingwood Prize for Juniors, for a paper on "Frictional Resistance in Artificially Roughened Pipes," presented each year to a junior in the society for a paper describing work with which the writer has been connected directly.

W. W. Horner, professor of municipal and sanitary engineering, Washington University, St. Louis, and F. L. Flynt, principal engineering aide, Hydraulic Section, U. S. Engineer Office, Rock Island, Ill., jointly—The Rudolph Hering Medal, for a paper describing the "Relation between Rainfall and Run-off from Small Urban Areas."

Honorary membership in the society was conferred on Dr. George S. Davison, of New York, Otis E. Hovey, of New York, the late Hunter McDonald, of Nashville, and J. R. Worcester, of Boston.

AWARDS OF THE AMERICAN INSTITUTE, NEW YORK CITY

THE annual meeting and dinner of the American Institute, New York City, was held at the Hotel Ambassador on February 3. On this occasion the gold medal of the institute, awarded for "distinguished contributions to the advancement of science which have a broad incidence on human welfare," was presented to Dr. William Crocker, director of the Boyce Thompson Institute for Plant Research at Yonkers, N. Y., in recognition of his "contributions to the knowledge of life processes in plants and for his unique leadership in the organization of plant research." Dr. Edmond W. Sinnott, professor of botany at Barnard College, Columbia University, made the presentation.

Fellowships of the institute were presented to Dr. Raymond L. Ditmars, curator of reptiles and mammals at the New York Zoological Park, and to Waldemar Kaempffert, science editor of *The New York Times* and president of the National Association of Science Writers. The fellowship was awarded to Dr. Ditmars "for his thirty-seven years of distinguished service in the care, understanding and interpretation of the reptile world and for his inspiring of youthful spirits with the zeal of the naturalist," and to Mr. Kaempffert in recognition of "his scholarly interpretation of scientific advances, for his editorial wisdom, for his adroit cultivation of the public mind toward a rational outlook and for his leadership in enforcing their social responsibilities upon scientists." George P. Brett, Jr., president of the Macmillan Company, made the presentation to Dr. Ditmars, and Dr. John H. Finley, editor of *The New York Times,* made the presentation to Mr. Kaempffert.

The Gold Medal of the American Institute may be awarded either to organizations or to individuals. In previous years it has been given to the Research Laboratories of the General Electric Company; to Dr. Oscar Riddle, of the Carnegie Institution; to the Bell Telephone Laboratories; to Dr. John C. Merriam, president of the Carnegie Institution; to Dr. Carl D.

Anderson, of the California Institute of Technology; to the late Dr. Julius A. Nieuwland, of Notre Dame University, and to Dr. E. V. McCollum, of the Johns Hopkins University.

THE SOUTHWESTERN DIVISION OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE eighteenth annual meeting of the Southwestern Division of the American Association for the Advancement of Science will meet in association with the Southwestern Section, Mathematical Association of America, at Albuquerque, New Mexico, from April 25 to 28. The University of New Mexico will be the host institution for this meeting. Dr. John D. Clark, University of New Mexico, is chairman of the general committee in charge of local arrangements.

On Monday evening, April 25, the John Wesley Powell Lecture will be given by Dr. E. R. Hedrick, provost of the University of California at Los Angeles, on "The Relation of Science to Economics and to War." This lecture is sponsored by the Southwestern Division as a courtesy to the community in which the meeting is held, and is open to the general public.

On Wednesday evening the presidential address will be delivered on the topic of "The Origin of Subterranean Carbon Dioxide" by Dr. F. E. E. Germann, University of Colorado.

On Thursday, April 28, an all-day excursion is planned which will be of general interest to all the sections of the division. The route of the excursion will include the historic camping site of Coronado, present-day and prehistoric Indian villages and some of the most interesting geological formations of the state. Much of the trip lies through national forests, and the Forestry Service staff, located at Albuquerque, is cooperating with the division in the matter of guides and transportation.

The opening general session of the meetings will b held on Monday morning at eleven o'clock, and se tional meetings start at two o'clock of the same da A feature of Monday afternoon will be the receptio tendered to the association and its guests by Dr. F. J Zimmerman, president of the University of Ne Mexico, at his home. Sigma Xi is planning a dinne and address for Tuesday evening, April 26. In addi tion several other sectional dinners and luncheons a being planned. Numerous scientific and commercia exhibits are being scheduled which will place more tha the usual emphasis upon this aspect of the program.

Titles of papers to be read at the meetings should b sent directly to the following section chairmen or se retaries:

Biological Science Section:
- *Chairman:* R. J. Gilmore, Department of Biology, Colo rado College, Colorado Springs, Colo.
- *Secretary:* R. H. Canfield, Las Cruces, New Mexico.

Mathematics Section (Southwestern Section, Mathematica Association of America):
- *Chairman:* R. F. Graesser, Department of Mathemati University of Arizona, Tucson, Ariz.
- *Secretary:* W. C. Risselman, Department of Math matics, University of California, Los Angeles, Cal fornia.

Physical Science Section:
- *Chairman:* W. M. Craig, Department of Chemistr Texas Technological College, Lubbock, Texas.
- *Secretary:* C. W. Botkin, Department of Chemistr New Mexico College of Agriculture and Mechan Arts, State College, New Mexico.

Social Science Section:
- *Chairman:* D. D. Brand, Department of Anthropolog University of New Mexico, Albuquerque, New Mexi
- *Secretary:* W. H. Hill, Department of Anthropolog University of New Mexico, Albuquerque, New Mexi

VEON C. KIECH,
Secretary

SCIENTIFIC NOTES AND NEWS

THE Bruce Gold Medal of the Astronomical Society of the Pacific has been awarded to Dr. Edwin P. Hubble, of the Mount Wilson Observatory. The Board of Directors of the society each year selects the recipient of the award from the nominations of the directors of six observatories: Greenwich, Paris, Córdoba, Harvard, Yerkes and Lick.

THE cross of the Order of Leopold II has been conferred by King Leopold III of Belgium on Dr. Hugh S. Taylor, chairman of the department of chemistry of Princeton University, in appreciation of his services to education while occupant in 1937 of the Francqui chair at the University of Louvain.

THE gold medal of the International Faculty Sciences, London, given in recognition of contributio to scientific advancement, was awarded on January to Dr. Ellice McDonald, director of the Biochemi Research Foundation of the Franklin Institute, for h contributions to cancer research.

DR. EDWARD L. KEYES, JR., professor of urology the Cornell University Medical School, in recogniti of his "outstanding service to humanity," was pr sented with the William Freeman Snow medal of t American Social Hygiene Association at the twent fourth annual dinner meeting in New York City February 3.

DR. THOMAS CHARLES POULTER, since 1936 director of the Research Foundation of the Armour Institute of Technology, Chicago, senior scientist and second-in-command of the Byrd Antarctic expedition of 1933–35, was presented on January 19 with a Congressional Medal "for scientific accomplishment unequalled in polar exploration." His special field was auroral and meteor observations and geophysics. A dinner in his honor was given in the evening at the Palmer House, at which, in the presence of four hundred people, addresses were made by Rear Admiral Hayne Ellis, commandant of the Ninth Naval District; Brigadier General Philip B. Peyton, commanding general at Fort Sheridan; Major Chester L. Fordney, U. S. Marine Corps; Brigadier General Charles Gates Dawes, formerly ambassador to England and formerly Vice-President of the United States; Dr. Arthur H. Compton, of the University of Chicago; James D. Cunningham, chairman of the board, and Dean Henry Townley Heald, acting president of the Armour Institute.

DR. DOUGLAS JOHNSON, professor of physiography at Columbia University, was elected a foreign member, and Dr. Eugene Van Cleef, professor of geography at the Ohio State University, a corresponding member of the Geographical Society of Finland on the occasion of the celebration of the fiftieth anniversary of the founding of the society.

DR. E. D. MERRILL, Arnold professor of botany, administrator of botanical collections and director of the Arnold Arboretum of Harvard University, has been elected an Académico Honorario of the Instituto del Museo, Universidad de la Plata, Argentina.

AT the Indianapolis meeting of the American Society of Plant Physiologists two Charles Reid Barnes life memberships were awarded, one to Dr. H. L. Shantz, of the U. S. Department of Agriculture, already recorded in SCIENCE, and one to Dr. Alexander P. Anderson, of the Tower View Laboratories, Red Wing, Minn. Professor H. H. Dickson, of Trinity College, Dublin, who developed the cohesion theory of sap ascent in plants, and Dr. Alfred Ursprung, of Fribourg, Switzerland, who developed accurate methods of measuring osmotic relations in plant cells, were elected corresponding members.

AN American Academy of Dermatology and Syphilology was organized at a meeting held in Detroit on January 14 and 15. The following officers were elected: Drs. Howard Fox, New York, *president;* Paul A. O'Leary, Rochester, Minn., *vice-president;* Earl D. Osborne, Buffalo, *secretary,* and Clyde L. Cummer, Cleveland, *treasurer.*

CROSBY FIELD, president of the FlakIce Corporation, Brooklyn, was elected at the recent New York meeting president of the Society of Refrigerating Engineers to succeed H. M. Williams.

THE new president and council of the Society of American Foresters, elected for the two-year period 1938–39, have taken office. Dr. Clarence F. Korstian, director of the Forest of Duke University, is president; William G. Howard, director of the Division of Lands and Forests of the Conservation Department, Albany, New York, is vice-president.

AT the annual meeting of the Harvey Society, held on January 28, 1938, the following officers were elected for the year 1938–1939: *President,* Dr. Philip E. Smith; *Vice-president,* Dr. Herbert S. Gasser; *Treasurer,* Dr. Kenneth Goodner; *Secretary,* Dr. McKeen Cattell (reelected). Dr. Hans T. Clarke, Dr. James D. Hardy and Dr. William S. Tillett were elected members of the council.

DR. HENRY F. HELMHOLZ, Rochester, Minn., was elected president of the International Congress of Pediatrics, which met in Rome in September, and Dr. Kenneth D. Blackfan and Dr. Charles F. McKhann, both of Boston, have been elected general secretary and treasurer, respectively. The next meeting will be held in Boston, probably in August or September, 1940.

The Cornell Alumni News reports that Dr. Wilder D. Bancroft, professor of chemistry emeritus, is recovering from serious injuries received when struck by an automobile on January 12.

DR. ROBERT WILLIAMS WOOD, professor of experimental physics at the Johns Hopkins University since 1901, and Dr. Herbert Spencer Jennings, professor of zoology since 1906, will retire in June. President Bowman has invited them to continue their researches at the university and has obtained grants to aid them in their experiments.

PROFESSOR WILLIAM N. BARNARD has been appointed director of the Sibley School of Mechanical Engineering in the College of Engineering of Cornell University. He has been head of the department of mechanical engineering for eighteen years and during the past year and a half has been acting director of the school.

DR. JOHN MULHOLLAND has been appointed assistant dean of the College of Medicine of New York University. He succeeds Dr. Currier McEwen, who was made dean after the death of Dr. John Wyckoff in June.

DR. KARL KORNBLUM, assistant professor of radiology in the Graduate School of Medicine of the University of Pennsylvania and director of the x-ray-radium department of the Graduate Hospital, has been elected professor of roentgenology in the Jefferson Medical College of Philadelphia.

DR. SIEGFRIED VON CIRIACY-WANTRUP, authority on land utilization, has been appointed to a professorship at the Giannini Foundation of the University of California. He has been working in the College of Agriculture for the past six months under a fellowship of the Rockefeller Foundation.

PROFESSOR ALBERT E. PARR, a graduate of the Royal Observatory of Oslo, who has been director of marine research at Yale University and curator of the Bingham oceanographic collection, has been appointed director of the Peabody Museum. He had been associated with the Marine Biological Station of Norway and the Norwegian Bureau of Fisheries, joining the faculty of Yale University in 1927. He succeeds Dr. Richard S. Lull, Sterling professor of paleontology, who has been acting director since his retirement from the directorship of the museum in 1936. Dr. Lull has been for thirty years a member of the faculty.

W. M. H. GREAVES, chief assistant at the Royal Observatory, Greenwich, has been appointed Astronomer Royal for Scotland and professor of astronomy in the University of Edinburgh. He succeeds Professor R. A. Sampson, who recently retired.

AT the Office of Cancer Investigations of the U. S. Public Health Service at the Harvard Medical School, Dr. H. B. Andervont, biologist since 1930, has been promoted to the rank of senior biologist. Dr. M. J. Shear, biochemist at this station since 1931, has been promoted to the rank of senior biochemist.

DR. E. W. RUSSELL has been appointed Goldsmiths' Company's soil physicist at the Rothamsted Experimental Station, in succession to Dr. G. W. Scott Blair, who has been appointed head of the department of dairy chemistry at the National Institute for Research in Dairying at Reading.

DR. J. A. PRESCOTT, professor of agricultural chemistry in the University of Adelaide, has been appointed director of the Waite Institute, succeeding Professor Richardson, who has been appointed deputy chief executive officer of the Commonwealth Council for Scientific and Industrial Research.

DR. ARTHUR H. COMPTON, professor of physics at the University of Chicago, has accepted the Protestant cochairmanship of the National Conference of Jews and Christians. Dr. Compton succeeds the late Newton D. Baker. The Catholic cochairman is Professor Carlton J. H. Hayes, of Columbia University, and the Jewish cochairman is Roger W. Straus, of New York, industrialist and philanthropist.

DR. C. I. BLISS, who returned to America in December, following two years of study with Professor R. A. Fisher at the University of London and two years as foreign specialist at the Institute of Plant Protection in Leningrad, is spending the months of February and March at the Lilly Research Laboratories in Indianapolis as consultant on the design and analysis of biological assays.

DR. PHILIP BARD, professor of physiology at the School of Medicine of the Johns Hopkins University, will deliver the fifth Harvey Society lecture of the current series at the New York Academy of Medicine on February 17. He will speak on "Studies on the Cortical Representation of Somatic Sensibility."

THE Jesup Lectures, given under the auspices of the department of zoology at Columbia University, will be delivered by Dr. John H. Northrop, of the Rockefeller Institute, on February 25, March 1, 4, 8, 11 and 15, in Schermerhorn Hall, at 5 P. M. The subject is: "Chemistry of Proteolytic Enzymes and Bacteriophage."

THE fourteenth Ludvig Hektoen Lecture of the Frank Billings Foundation of the Institute of Medicine of Chicago will be delivered by Dr. William C. Rose, professor of biochemistry at the University of Illinois, on February 25 at the Palmer House. His subject will be: "The Physiology of Amino Acid Metabolism."

RECENT visitors at the School of Tropical Medicine of the University of Puerto Rico include: Dr. E. G. Nauck, of the Institute of Tropical Medicine at Hamburg, who stopped at San Juan for a few days, *en route* to Santo Domingo, Venezuela and Colombia. Dr. Elíseo Ramírez, who is at present director of the Laboratory of the Federal Health Department and professor of pathology at the Military Medical School of Mexico City and who is to direct the new Institute of Tropical Medicine there, arrived with Dr. P. Varela, formerly fellow of the Medical Section of the League of Nations. Dr. Dana W. Atchley, of the department of preventive medicine, and Dr. Ramón Castroviejo, of the Institute of Ophthalmology of the College of Physicians and Surgeons in New York City, attended as guest speakers the annual meeting of the Puerto Rico Medical Association held on December 17, 18 and 19. Dr. Atchley spoke at a weekly conference of the school on "The Rôle of Sodium in Clinical Medicine." Dr. Robert M. Yerkes, director of the Laboratories of Primate Biology of Yale University, arrived on a short visit to see the work of the Free Range Primate Colony on the Island of Santiago. He lectured at the school on "Morphine Addiction in the Chimpanzee." Dr. H. D. Tate, of the Bureau of Animal Industry at Washington, is at the school for the winter working on the problem of tick eradication. Dr. Donald S. Martin, of Duke University, is spending the winter in Puerto Rico and will work with Dr. Arturo L.

Carrión, of the department of mycology and dermatology of the school.

The ninth annual meeting of the American Association of Physical Anthropologists will be held at the University of Pittsburgh in conjunction with the American Association of Anatomists from April 14 to 16.

DISCUSSION

"LODI MAN"

Rewritings in the press of the authenticated University of California release on "Lodi Man" have resulted in certain unintended emphases which the present statement is intended to correct. It may be said at the outset that the findings positively established relate to cultural sequence only. Conclusions as to geology, skeletal human type and age are as yet only tentative.

It has long been known that the region of Stockton, Lodi and Galt, and in part north to Sacramento, about the middle of the Great Valley of California, contained some distinctive archeological forms, such as crescentic obsidian blades and decorated clay balls. But interest at first was in typological rather than chronological problems. In 1929 Schenck and Dawson hesitantly distinguished an earlier and a later cultural horizon.[1] From about 1933 on, Sacramento Junior College, under the leadership of President Lillard, excavated in a series of mounds, and in 1936 published a compact report in which the two periods (plus a recent one) were positively separated.[2] though without the itemized grave-by-grave listing of artifacts which would have rendered an independent check possible.

During 1936–37, R. Heizer and A. Krieger were in charge of intermittent explorations of the area for the University of California. Heizer had previously participated in the Sacramento College diggings, while a student there. In the summer and fall of 1937 he headed a group which excavated further, partly with support from Mr. and Mrs. Beverly Blackmer, whose assistance is gratefully acknowledged. President Lillard also generously put at Heizer's disposal the full field notes recorded during several years of work by Sacramento, thus multiplying several fold the data available to the university. An analytic comparison of individual grave finds established the following conclusions:

There were two native cultures in the area. Certain types of artifacts occur only with certain others. A second class of artifact types occur only with one another, not with types of the first class. The second class is occasionally found with Caucasian objects, hence this culture continued into the historic period and must be the later of the two. Most sites contain material of only one of the two cultures, which are therefore mainly mutually exclusive and separate in time. In a few sites both cultures occur, but in separate graves. These two classes of graves differ less in absolute depth or superposition—all the culture-bearing levels are rather shallow—than in the soil in which they were made and with which they were refilled; indurated clay for first-period graves, alluvium for second. First-period burials are normally extended, second-period flexed.

The cultural differences are of three degrees. First, there are fundamental types exclusively peculiar, such as pencil-shaped slate rods in the earlier culture and clay balls and magnesite cylinders in the later. Second, there are types which carry through, such as charmstones, beads of *Olivella,* disks of *Haliotis,* but with a consistent alteration of form or subtype. Third, a minority of artifacts are common to the two cultures. So far as the purely cultural evidence goes, there may accordingly have been a time gap between the two cultures, but need not have been one. It is conceivable that sites with a transitional culture may yet be discovered. The significance of the results lies in the fact of a cultural succession—the second time, only, that such a succession has been fully established in California—the first being the two (or three) horizons independently recognized in the Santa Barbara channel region by David Rogers and Olson. In Heizer's opinion it is the earlier of the two Lodi-Galt or delta cultures which has the greater affinity to the Santa Barbara cultures.

As regards racial type, the present preliminary indications are that skulls from graves of the earlier culture are narrower and of "pseudo-Australoid" or "Palaeo-American" type. However, the indurated clay matrix has prevented intact recovery of many of the earlier skulls. Of those which are measurable, some are in Berkeley, some in Sacramento, some in Washington. They have not been assembled for systematic study and comparison with the later-period skulls. Until this has been done, all findings as to racial type should be regarded as impressionistic and provisional. That two races should eventuate seems likely enough in view of Gifford's having previously separated out a minority "Buena Vista type" from the Indian skulls of the San Joaquin valley.[3]

Another problem is raised by the soil formations involved. This is now being investigated by C. O. Sauer, geographer, and H. Jenny, soil chemist, at

[1] *Univ. Calif. Publ. in Am. Arch. Ethnol.,* 25: 289–413; see especially p. 402.

[2] J. B. Lillard and W. K. Purves, *Sacramento Junior College, Dept. Anthrop. Bull.,* 1.

[3] *Univ. Calif. Publ. in Am. Arch. Ethnol.,* 22: 217–390, 1926, especially pp. 246–7.

Berkeley. Their first-inspection impression at the crucial two-culture site was that a relation of formations of the kind there encountered would ordinarily involve a considerable period, possibly of some thousands of years. It is on this indefinite preliminary reaction that the press statements are based that "Lodi man may be" 15,000 years old. He *may* be, in the sense that the required laboratory tests and further field observations have not yet been completed, and the age is therefore as yet unknown. As an anthropologist, I should be surprised if the soil structure compelled belief in the lapse of a very long time, because after all the two cultures are generically similar.

Similarly as to skeletal mineralization, which seems much greater in the early-period bones. It is notorious that this may proceed at highly variable rates. Moreover, no quantitative determinations are yet available. While this is again a highly promising lead, no reliable findings bearing on age have yet been made from degree of mineralization.

The San Francisco Bay shellmounds, fifty to a hundred miles downstream, probably contain a related problem. Some of them undoubtedly go back to a considerable age. While Schenck has shown that some may not be as old as at first estimated—3,000 to 4,000 years—he has not proved that they are all younger. It is also inherently unlikely that culture stood stock-still during the whole period of accumulation of these large middens. While nearly all the more important shellmounds have now been obliterated, a considerable body of objects and data on them has accumulated at the university during the past 35 years. A beginning has recently been made of analyzing these data by the same method as used by Heizer: recurrent associations of finds. While it is probable that culture change on the Bay has been relatively slow, else differences would long since have obtruded themselves, such sequences as there were should however be determinable, and will then presumably correlate with those established for the Stockton-Sacramento and Santa Barbara areas.

Since my name has been brought into the press reports, it is only fair to state that my connection with the work near Lodi has been wholly advisory, and mainly cautionary. I have not even seen what is regarded as the type site. The first pertinent observations were made by Dawson, the first recognition of a possible culture succession by Schenck. Lillard and his associates have assembled much the largest body of exact archeological data. Heizer has been responsible for the most recent investigations and archeological interpretation. The geomorphologists, soil chemists and physical anthropologists are still to be heard from.

A. L. KROEBER

UNIVERSITY OF CALIFORNIA

A NEW DISTURBANCE OF RED PINE

RED or Norway pine (*Pinus resinosa* Ait.) has been regarded as a particularly suitable species for reforestation in the northeastern and lake states. The susceptibility of the more valuable white pine (*Pinus strobus* L.) to tip weevil and blister rust and the relative freedom of red pine from pests have caused the latter species to be given preference in many cases. The investigations now being carried on by the writer upon an unreported disturbance of red pine indicate that its freedom from disease is more apparent than real in many sections of the northeastern United States.

The external symptoms of the disease were first noticed by James A. Brock, assistant superintendent of the Rochester Municipal Watershed, in a plantation of young red pine in Ontario County, N. Y., during the summer of 1933. Since then the writer has found it or had reports of it throughout most of New York State, Connecticut and two counties in southeastern Pennsylvania. In gross aspect the symptoms resemble some types of insect injury, as the most conspicuous external characters are the extra-seasonal growth of one or more lateral buds in the terminal bud-cluster and the subsequent "forking" of the tree. The extra-seasonal growth of the lateral buds begins in June or July of the year that they are set, and may continue through August in the region of Hemlock Lake, N. Y. One or more buds are formed at the tip of these precocious shoots before growth ceases. The terminal bud of the parent shoot seldom takes part in this extra-seasonal growth and usually elongates at the normal time the following year. Since the abnormal or precocious laterals assume a more vertical position than is normal, the subsequent growth of the terminal bud causes a forked appearance of the tree. In some cases the original terminal may be forced to take the position of a lateral. This phenomenon, although undesirable, *per se* might be of no great consequence were it not for the fact that organic union of the wood usually fails to take place between the forked members during the later growth of the tree. Dissection of a tree, in which forking had occurred, ordinarily showed that the forked branch had failed to unite on its upper surface with the bole of the tree. A resinous pocket or fissure, surrounded by discolored wood, usually occurs at these areas of non-union. Such defects afford an ideal environment for many species of fungi, some of which are known to be parasitic.

Representative plots from approximately 800 acres of red pine, ranging in age from 5 to 25 years, have shown forking in 68 to 94 per cent. of the trees. The affected trees seldom, if ever, remain permanently forked, as the more vigorous member of the fork assumes a completely vertical position after a few years' growth and the other member tends to take the position characteristic of a normal branch. Hence the

presence of the disturbance in a stand of red pine may not be evident to a casual observer until the invasion of parasitic fungi takes place.

Forking has been found on soils varying in pH from 4.5 to 7.5 and ranging in texture from sandy to clay loam. It has been observed in young natural reproduction as well as in plantations and in both mixed and pure stands. Investigations are being carried on in an effort to determine the primary cause and mechanism of forking. The evidence at hand strongly suggests a fungous origin.

JOHN AUSTIN JUMP

DEPARTMENT OF BOTANY,
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SELENIUM DEHYDROGENATION OF NAPELLINE

THE recent publication of Lawson and Topps[1] anticipates on one important point results obtained in this laboratory. These authors secured on selenium dehydrogenation of atisine, $C_{22}H_{33}O_2N$, a hydrocarbon $C_{17}H_{16}$, characterized by its picrate $C_{17}H_{16} \cdot C_6H_3O_7N_3$, orange needles, m.p. 129°, and its trinitrobenzene-derivative, yellow needles, m.p. 140°.

We[2] have obtained by selenium dehydrogenation of napelline $C_{22}H_{33}O_3N$ apparently the same hydrocarbon as picrate, orange needles melting at 130° (Found: C, 60 .95; H, 4.59; N, 9.78, 9.57. Calc. $C_{17}H_{16} \cdot C_6H_3O_7N_3$: C, 61.47; H, 4.23; N, 9.34;) The trinitrobenzenederivative crystallizes in dark yellow needles and melts at 138°.

Lawson suggested that the hydrocarbon is a substituted phenanthrene. Blount[3] has obtained a compound cevanthrol, $C_{17}H_{16}O$, by dehydrogenation of cevine, a veratridine derivative; x-ray measurements indicate the probability that cevanthrol is an alkyl phenanthrol. It is of especial interest to note that similarity of these reports, because a link between the aconitine and veratrine alkaloids is highly desirable.

E. F. ROGERS
WERNER FREUDENBERG

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LUTHER BURBANK

To this writer it has seemed that it is high time some one told the truth about Luther Burbank. I believe I have read every report about him of any importance that has ever been published, but they mostly consist of fairy tales, sentimental rot and propaganda. Some have tried to relate facts but let their emotions get the better of them. Others were frankly hostile, and therefore biased. Exceptions to the above, of course, are the limited writings of Hugo de Fries, Vernon Kellogg, David Starr Jordan and L. H. Bailey.

I have undertaken the task of ferreting out the facts about this man, whose name (no matter what we may think of him) is almost a household word in many languages.

I find it necessary to see all the catalogues and price lists he ever issued in order that I may abstract them and evaluate all the items. I have searched libraries and private collections from coast to coast and have succeeded in finding more than a hundred pieces, but I am sure there are many more still lost. Private collections are my best bet and I, therefore, appeal to readers of SCIENCE to send me anything they may have among their old papers. If requested to do so I will gladly return them.

W. L. HOWARD,
Professor of Pomology

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SCIENTIFIC BOOKS

The World Around Us. A Modern Guide to Physics. By PAUL KARLSON. 293 pages, 8 plates. New York: Simon and Schuster, 1936. $3.00.

THE rapid development of physics in the past few decades has stimulated the interest of the layman in this subject, and has created a demand for articles and books explaining in everyday language the achievements in physics to the general reading public. To appreciate these achievements it is necessary to know something more about physics in general, and that is exactly what Karlson realizes. The first chapters are devoted to the question of "matter and motion," electricity and light waves. The next chapter deals with the conceptions of relativity, and in the last two chapters he treats the topics of light quanta and the new ideas which have been introduced in the development of the quantum theory. The Bohr atom, waves of matter, artificial disintegration, the uncertainty principle and causality and probability are discussed, and with an outlook on the new picture of the universe the book closes.

The usual objections against writings of this type by the educated non-scientist are, that the arguments are not formulated precisely enough, and that the analogies necessary, to obviate mathematical formulae, are too far fetched. It is particularly important, for a book of this type, to use the same terminology as the

[1] *Jour. Chem. Soc.*, 1640, 1937.
[2] *Jour. Am. Chem. Soc.*, in press.
[3] *Jour. Chem. Soc.*, 414, 1936.

scientific texts written for experts so as to make it possible for the layman to understand the meaning of the new terms, and to really "guide" him to some more advanced study.

Dr. Karlson's book first appeared in 1934 in Germany. The American edition opens with the startling statement, "the four elements of the ancients were something like the players in a pinochle game; pure speculation brought them together in the first place and an over-dose of poetic frenzy or excessive juggling with unknown quantities was forever disrupting the harmony of the group." To a lawyer or banker who is just as logical in his own work as the physicist is in his research, such a statement will produce considerable antagonism. Leafing through the book it becomes clear that this edition was translated by some one who is either unfamiliar with modern phrases used in physics or with their proper English equivalents. One finds for crystal lattice the term "trellis" (p. 10, etc.), for photographic plate, "dry plate" (p. 17), for vacuum pump, "mercury air pump" (p. 31), for energy of motion or kinetic energy, "movement energy" (p. 34), for the second law of thermodynamics, the "second fundamental proposition" (p. 44), for fluctuation, "oscillations" p. 50). But even the German is sometimes translated wrongly, as on page 91 where instead of "they" we find "you" (both correspond to the German sie!), and consequently the meaning is entirely misunderstood. Instead of loosened up, we find "relax" (p. 104); instead of filament generators, "heat generators" (p. 108); instead of image plane, "visual plane" (p. 129); instead of earth, "soil" (p. 186). Many other examples could be quoted. When we found that there occurred some serious mistakes in the discussion of physics, we decided to consult the German edition so as to find out whether these mistakes are contained in the original, or are peculiar to the American edition.

Let us say at once that Dr. Karlson's original German text fulfils every requirement one could set for a book of this type. There are hardly any mistakes in the statements of scientific principles (and of the abundance to be found in the American edition we will only quote a few: page 8, "on the wire leading from the *positive* terminal *twice* as much gas is formed as on that leading from the *negative*!"; page 29, "a lot of electrons would form a sort of x-ray in the neighborhood—a gamma radiation"; page 62, "this light (produced on the glass of an x-ray tube) consists of rays that go in straight lines perpendicular to the cathode —cathode rays"; page 79, statement connecting charge with osmotic pressure is wrong; page 84, "the swimmer rule: if a little man were to swim *north* with the electric current"—! and page 120, "it is only a difference in wave lengths which determines whether a given vibration shall be heard (!sic) by the ear as sound—seen by the eye as light.") All these wrong statements and many others do not at all occur in the German edition. It is really an astonishing and remarkable achievement in teaching that Dr. Karlson was able in a book of this type not only to give an introduction to classical physics and to give just the right amount of material necessary to introduce the most modern concepts, but also to choose analogies with such admirable skill that it is hardly possible to find any faults with most of them. It is also remarkable with what subtle timing he changes from analogy and introduction to scientific reasoning. In this way he has made it possible for a layman to remain interested, informed and stimulated. By using dialogues and discussions with characters of the past which are introduced in the most charming way, Dr. Karlson was able to bring to the mind of the newcomers the method of development in physics leading to our present outlook. The index in the German edition is something unique in itself. It is really a dictionary, giving at the same time the definition of each term and the connection in which it is used in the text. Finally the German edition contains a "pedigree" of the leading ideas in physics, showing the interlinkages between the different ideas as they developed.

All this has been eliminated in the American edition. Instead of the warm enthusiasm with which Dr. Karlson has introduced his fictitious characters in the different discussions, they have acquired a certain impishness, a superficial smartness which leads to discussions which have no connection with the topic or the chapter. (Needless to say the introductory statement quoted above does not correspond to the German original.) The clear and instructive summaries of any theory mentioned, which is so characteristic for Karlson's book, are in most cases omitted in the American edition. It reminds the reviewer of some of the screen versions taken from well-known classics which are supposed to cater to popular taste, and retain hardly anything of the spirit of the original text.

We think that in justice to the author and to the American public which is intensely interested in modern developments in physics, a new edition of Dr. Karlson's book should be prepared, eliminating the mistakes, the wrong terminology and restoring the charm and knowledge of the German original. Such an edition will not only help the layman to understand modern physics, but it will be an invaluable help to physicists and teachers of any science class in the presentation of the cultural aspects of physics.

KARL LARK-HOROVITZ

PURDUE UNIVERSITY

REPORTS

THE REINSTATEMENT OF PROFESSOR SCHAPER[1]

THE University of Minnesota was founded in the faith that men are ennobled by understanding; it is dedicated to the advancement of learning and the search for truth; it is devoted to the instruction of youth and the welfare of the state. These purposes, carved in stone upon the facade of its most stately building, embody the tradition of scholarship upon which rests the development of higher education and the continuous progress of democratic society.

It is this tradition that sustains the human mind and spirit when beset by human passions and prejudices. It is to this tradition that the board of regents of the University of Minnesota reaffirm its adherents. In so doing it reiterates its acceptance of the corollary principles of academic freedom. The board of regents of the University of Minnesota bears witness to its faith by entering upon its records the following statement concerning academic freedom:

(1) The University of Minnesota should not impose any limitation upon the teacher's freedom in the exposition of his own subject in the classroom or in addresses and publications.

(2) No teacher may claim as his right the privilege of discussing in his classroom controversial topics that are not pertinent to the course of study that is being pursued.

(3) The University of Minnesota should not place any restraint upon the teacher's freedom in the choice of subjects for research and investigation undertaken on his own initiative.

(4) The University of Minnesota should recognize that the teacher in speaking or writing outside of the institution upon subjects beyond the scope of his own field of study is entitled to the same freedom and is subject to the same responsibility as attach to all other citizens but in added measure.

(5) It is clearly understood that the University of Minnesota assumes no responsibility for the views expressed by members of its staff, and the faculty members themselves should, when necessary, make it clear when they are expressing only their personal opinions.

(6) If the conduct of a teacher in his classroom or elsewhere should give rise to doubts concerning his fitness for his position, the question in all cases should be submitted first to a committee of the faculty and in no case should any member of the teaching staff be dismissed before the normal termination of his period of appointment without full and open hearing before the board of regents, should he desire it, and only upon sufficient notice.

The board of regents sitting in 1938 recognizes with regret and not in a spirit of condemnation of its predecessors that periods of national crisis are characterized by wide-spread loss in social perspective and a strain upon the values that prevail when conditions are more nearly normal.

[1] Resolution adopted by the Board of Regents of the University of Minnesota on January 28, 1938.

It would also affirm in these calmer days and against another day of storm and stress that in times of crisis the need for adherence to accepted values and traditions and procedures, especially by institutions of higher education, is most necessary. It recognizes in retrospect that conditions in the fall of 1917 were such that seemingly fundamental differences in opinion were not quickly reconciled or adjusted.

When America entered the war after nearly three years of neutrality and free discussion, those who had vigorously upheld the cause of the Central Powers were expected to reverse at once emotional and intellectual attitudes to which of right they had given free play. Those who had favored the Allies faced no such difficulty, often could not recognize its existence and added their impatience to increase the difficulties of those who sought at the same time to save their self-respect and prove their loyalty.

It was such conditions, with the consequent effects on all parties concerned, that furnish in part the background for the action of the board of regents when on September 13, 1917, it passed a resolution dismissing William A. Schaper from the University of Minnesota.

This action of the board was initiated by a letter of the Minnesota commission for public safety, advising the president of the board that it was claimed by informants of that commission that Professor William A. Schaper was a rabid pro-German. There was a summary examination of him before the board on that day. No record of the proceedings other than the above resolution was made.

Numerous efforts have been made in the last twenty years by members of the staff, former students of Professor Schaper and alumni of this university to reopen the case. The matter, however, did not receive the attention of this board until the letter of Honorable Elmer A. Benson, governor of this state, addressed to Regent Lewis E. Lohmann, was presented to this board on December 17 last, requesting that the resolution of September 13, 1917, be rescinded and that Professor Schaper be invited to return to the university.

This board finds as follows:

Professor William A. Schaper was made full professor at the University of Minnesota in 1904, after having served three years as assistant professor. At the time of the adoption of said resolution, he was the head of the department of political science and filled that position with distinction.

He was not furnished with a copy of the alleged information against him.

No charges were made against him that might have been considered by a faculty committee and, therefore, none was considered by such a committee.

None of the charges were specified except as above stated.

He was not confronted with his accusers.

He was not given sufficient time or opportunity to meet the charges, nor to engage counsel for his defense.

He was dismissed on the eve of the commencement of the then academic year after being paid only one month's salary for that year.

This board finds that the dismissal was without due process, and, therefore, unjustified; *therefore, be it*

Resolved:

1. That the action of the board in adopting the resolution of Sept. 13, 1917, terminating the relation existing between Professor William A. Schaper and this university be, and it hereby is, in all things rescinded, and the said resolution be, and hereby is, in all things expunged from the minutes and records of this board.

2. That Professor William A. Schaper be reinstated to the faculty of this university, with the rank of professor of political science emeritus.

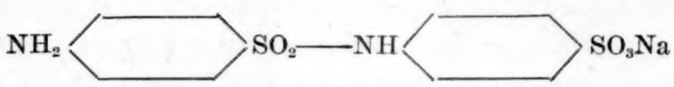
3. That Professor William A. Schaper be paid, out of the funds of this university, the sum of $5,000 in reparation of his loss of salary for the academic year 1917–18, and said sum is hereby appropriated for such purpose.

4. That a copy of this resolution be transmitted to Professor William A. Schaper by the secretary of this board.

SPECIAL ARTICLES

THE TREATMENT OF CANINE DISTEMPER WITH A CHEMOTHERAPEUTIC AGENT, SODIUM SULFANILYL SULFANILATE[1]

IN the following account, a preliminary report is given of results obtained from the treatment, with a chemotherapeutic agent, of animals infected with the virus of canine distemper. The compound used was sodium sulfanilyl sulfanilate and was prepared by the Calco Chemical Company. It has the following formula:



This compound is a white crystalline substance highly soluble in water and of neutral reaction. It is readily absorbed by way of the gastro-intestinal tract and has little or no toxicity for small animals in doses equivalent to one gram per kilogram of body weight. Ferrets, rabbits and cats have received one gram per day for periods as long as two weeks without loss of weight, appetite or other untoward symptoms.

An 8-kilogram dog was injected intravenously with 1.9 grams of sodium sulfanilyl sulfanilate, and the continued presence of the drug in the blood and the rate of its excretion in the urine were studied. The results are shown in Table I.

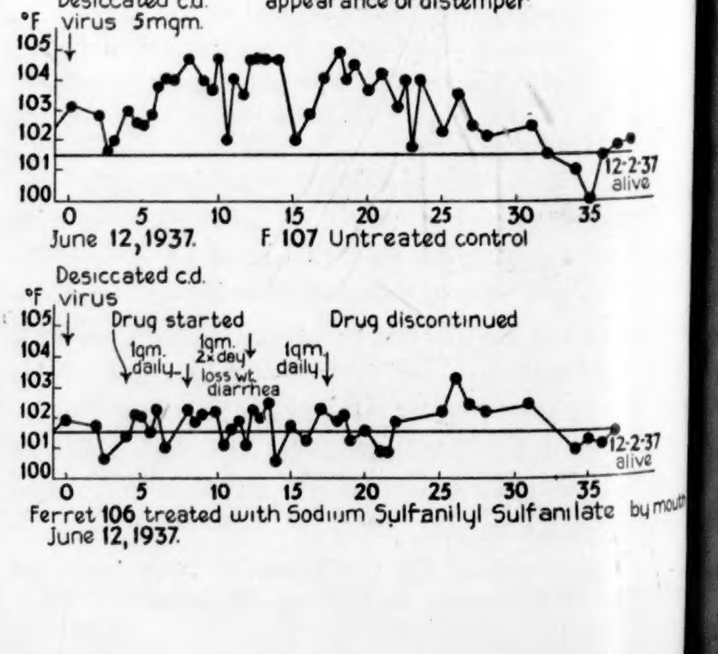
TABLE I

Time after administration of blood sample	Gamma of drug per ml of blood	Total amount of circulating drug
5 minutes	320	1.28 grams
2 hours	30	0.12 "
20 "	0	0.00 "
Time of urine sample	Volume of urine	Grams of drug recovered
20 hours	340 ml	1.5
44 "	300 "	0

The above table indicates that the chemical circulates in the blood in high concentration for relatively short periods of time, and that a large dose was completely excreted in the urine within twenty-four hours. The animal manifested no symptoms of intoxication and suffered no apparent bad after-effects from the treatment.

The therapeutic action of the drug has been tried on animals infected with the virus of canine distemper. In ferrets experimentally given this disease, sodium sulfanilyl sulfanilate has been found to have a remarkable therapeutic effect. It both prevents the development of the disease in animals treated within the incubation period, and cures the disease promptly in animals treated after the first rise in temperature and the appearance of symptoms. The action of the drug is equally efficacious whether dried living distemper vaccine virus Lederle is used for infection or fresh virus-containing filtrate obtained from dogs suffering from the spontaneous disease. The following chart illustrates the course of experimental canine distemper in two ferrets, one a control and the other treated with sodium sulfanilyl sulfanilate. Both ferrets were inoculated subcutaneously with 5 mgm of dried living canine distemper virus Lederle. Treatment was started four days after inoculation and before the appearance of symptoms. The treated animal was given 1–2 gms of the drug daily.

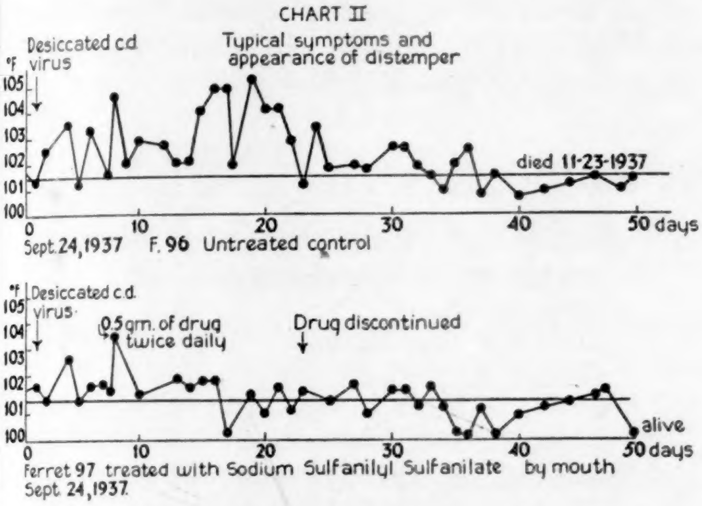
CHART I



[1] From the Department of Medicine and the Department of Animal Care, College of Physicians and Surgeons, Columbia University, and the Presbyterian Hospital, New York City.

Two additional ferrets were inoculated subcutaneously with 5 mgm of dried living canine distemper virus Lederle. Treatment was started on the eighth day after inoculation. Both animals developed fever on this day, the temperature of the control being 104.6° F. and that of the treated animal 104° F. The temperature of the treated ferret dropped to normal within twenty-four hours and the animal continued without symptoms thereafter. The untreated control displayed the characteristic symptoms of distemper in the ferret induced by inoculation of living distemper vaccine virus. The course of the disease in the two animals is shown in Chart II.

CHART II



Following the successful treatment of ferrets inoculated with canine distemper vaccine virus, the therapeutic action of the drug was tested on ferrets inoculated with canine distemper virus freshly prepared from dogs in the acute early stage of the disease.

CHART III

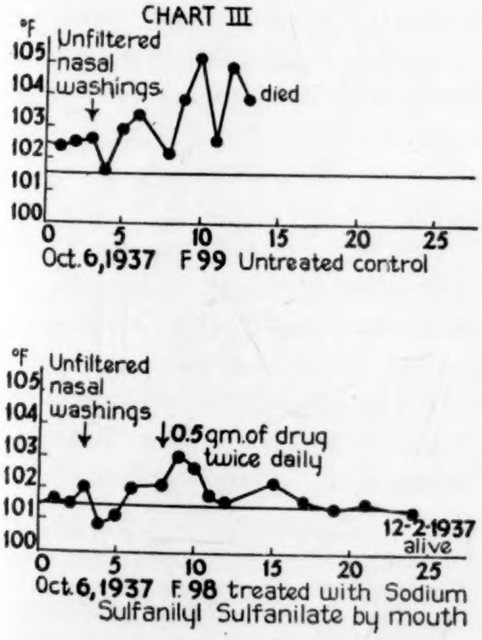
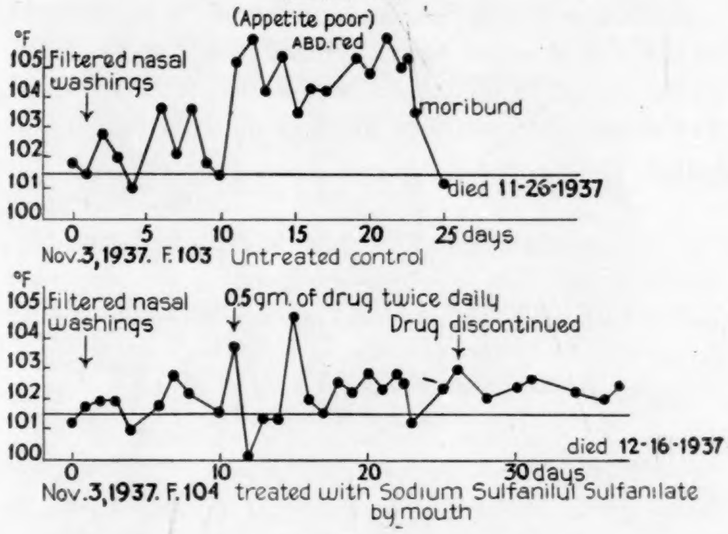


Chart III illustrates the course of the disease in two ferrets inoculated subcutaneously with unfiltered nasal washings of a dog suffering from distemper. The treated animal received orally 0.5 gm twice daily. The treatment was started on the fifth day following inoculation.

Two additional ferrets were next inoculated subcutaneously with the nasal washings of a dog in the acute stage of distemper. In this instance the washings were filtered free from bacteria by passage through a Berkefeld N filter. Treatment was started on the tenth day following inoculation. Symptoms of the disease first appeared on this day, the temperature of the control rising to 105° F. and that of the treated animal to 103° F. The treated animal received 0.5 gm of the drug twice daily. The results of the experiment are shown in Chart IV.

CHART IV



These experiments clearly indicate that sodium sulfanilyl sulfanilate, when administered to ferrets experimentally infected with canine distemper, both prevents the disease when given before the appearance of symptoms, and cures the disease promptly when administered shortly after the development of characteristic symptoms and fever. There is some evidence to indicate that serious secondary bacterial infection accompanying the distemper may destroy the favorable action of the drug. One treated ferret died twenty-five days after the discontinuance of the drug. At autopsy consolidated areas were found in the lungs, the exact nature of the disease not being apparent. Whether this animal died because of latent activity of the virus or was reinfected from a nearby diseased ferret is unknown.

Treated ferrets remain remarkably free from symptoms, maintain a good appetite and in general gain weight. No toxic manifestations were observed with a dosage of 1 gm daily. When the dose was increased to 2 gms daily, diarrhea and loss of weight appeared.

We have also given the drug a short clinical trial in spontaneous canine distemper in dogs. So far the effects seem to be of equal value to those observed in the experimental disease. Of twenty-eight animals treated at varying stages of the disease twenty-six have recovered. Symptoms and fever disappear rap-

idly and the appetite promptly returns. The animals remain well after the cessation of treatment. One animal treated on the fifth day of the disease recovered within forty-eight hours and thereafter remained well. Of the two fatal cases one animal died and the other was sacrificed. Both were in an advanced stage of the disease when first treated and had already developed severe secondary pulmonary infection. The amount of drug administered to dogs has been 1 gm twice daily. Eighteen cats suffering from a spontaneous disease commonly known as cat distemper or influenza have also been treated with the drug. Its effect in this condition is in all respects similar to that in canine distemper.

Sodium sulfanilyl sulfanilate therefore appears to be the first chemical agent to have such definite therapeutic action in an infection due to a filtrable virus. The range of its activity in virus diseases remains to be explored.

A. R. DOCHEZ
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THE EFFECT OF LIGATION OF THE LUMBOADRENAL VEINS ON THE COURSE OF EXPERIMENTAL DIABETES IN DOGS AND CATS[1]

HOUSSAY[2] was able to ameliorate the course of pancreatectomy diabetes by hypophysectomy. Subsequently, Long[3] produced a similar effect on depancreatized dogs and cats by complete adrenalectomy and maintenance with cortin. A new method for the alleviation of diabetes and some results obtained with this procedure are presented in this report.

In 20 cats and 4 dogs the lumboadrenal veins were ligated proximally and distally to the adrenal gland and the entire pancreas simultaneously removed. It should be emphasized that these animals at no time received either insulin, cortin or sodium chloride therapy. That the intensity of the diabetes is greatly diminished by the ligation of the lumboadrenal veins is shown by the following:

(1) *Survival.* The survival of completely depancreatized dogs and cats is usually less than 7 days. As a result of adrenal vein ligation, the range of survival has been increased in cats to 11–98 days; the average is about 20 days, excluding the cat living 98 days. The period of survival of dogs to date is 18, 11, 35 and 8 days. The first animal is still alive. The last 3 succumbed. However, the death of the dog on the eighth day was due to a post-operative complication and not diabetes.

(2) *Chemical Studies.* The values for blood sugar, blood fat, glycosuria and ketonuria of these dogs and cats are much lower than those observed in depancreatized animals without ligation of lumboadrenal veins. Occasionally marked hypoglycemia was observed, and in two cats fasting rendered the urine free of sugar.

The D : N ratio was well below the value of 2.8, characteristically associated with pancreatectomy diabetes. Respiratory quotients above 0.70 were obtained in every animal studied. The changes in carbohydrate metabolism are not secondary to alterations in blood total base. In many of the animals, the blood electrolytes were normal in concentration.

Gross and microscopic autopsy findings revealed complete absence of pancreatic tissue. Histological examination of the adrenal and pituitary glands are being made to determine the involvement of these organs.

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[1] This research was aided by a grant from the National Research Council.

[2] B. A. Houssay, *Am. Jour. Med. Sci.*, 193: 581, 1937.

[3] C. N. H. Long, *Medicine,* 16: 215, 1937.

THE LIQUEFACTION OF SPONTANEOUS TUMORS OF THE MAMMARY GLAND IN MICE BY HEPTYL ALDEHYDE[1]

RECENT data[2] have shown that certain characteristics of spontaneous tumors of the mammary gland in mice may be influenced by the daily administration of the true oil of gaultheria in the diet of those mice showing such neoplasias. These effects have to do with the clinical course and histological appearance of the tumors. It has been demonstrated that in early cases the connective tissue of the tumor seems to have been materially enhanced by a such a treatment. Similar results could not be produced by the use of redistilled synthetic methyl salicylate.[3] In an attempt to isolate the active agent of the true oil which had the above inhibitory action on spontaneous tumors, the true oil was subjected to fractional distillation. From this work, it was shown that the active inhibitory agent was contained in the low boiling point fraction, that is, in that fraction which distilled over below the boiling point of methyl salicylate.[4] It was demonstrated that the low fraction had a pronounced effect on: (1) the

[1] This experiment has been made possible by grants from the International Cancer Research Foundation and the Anna Fuller Fund. Acknowledgment is also due to the Fluid Research Funds of Yale University School of Medicine. Dr. W. Bergmann has very kindly redistilled the commercial C.P. heptyl aldehyde used in this experiment.

[2] L. C. Strong, *Am. Jour. Cancer,* 28, 550, 1936.

[3] *Idem., Am. Jour. Med. Sci.,* 192, 546, 1936.

[4] *Idem.,* (in press) *Am. Jour. Cancer.*

slowing up of the growth rate of tumors, with complete regression in 4 out of 34 animals; (2) an increase in the survival time of the mouse after the onset of cancer, and (3) gross and histological alterations in the tumors themselves. These changes in the tumors themselves were (1) softening and in some cases, (2) complete liquefaction. The action of the low fraction appeared to be more pronounced than the action of the true oil of wintergreen.

Since heptyl aldehyde is an ingredient of the low fraction, it was decided to put mice bearing spontaneous tumors of the mammary gland on a diet containing this chemical. The heptyl aldehyde was purchased from the Eastman Kodak Company and redistilled. Only that part which distilled at 152° C. was used in this experiment. A very pronounced softening and liquefaction of the tumors occurred in the mice receiving heptyl aldehyde in an otherwise normal or standard diet. Liquefaction was so extensive that drainage through a hypodermic needle under sterile conditions was easily accomplished. Six of the first twenty-five mice placed on the heptyl aldehyde treatment completely regressed their tumors. Liquefaction and regression of tumors never occurred in 120 individuals which served as controls. Samples of the drained-off liquid were tested by Dr. C. G. Burn and found to be sterile.

The present investigation is of interest, since it opens up the question that spontaneous tumors, in mice at least, may eventually be controlled by chemotherapy.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

CLEAVING NEMATODE EGGS AS RESEARCH AND CLASSROOM MATERIAL

WHILE any number of marine forms supply living cleavage material during the summer months, it has been difficult to find material which can be used for either research or teaching purposes during the other seasons. One of the most favorable of plant materials is the stamen hairs of Tradescantia, but Tradescantia presents the difficulty of not flowering consistently through the winter months. The work of Rugh[1] on the induction of ovulation in amphibia now offers a source of cleaving eggs during the entire school year and has become an invaluable addition to many embryology courses. But amphibian eggs, because of their dense pigmentation, are not well adapted to the study of many features of the division process. These disadvantages may be overcome by the use of the eggs of small nematodes. American workers have so far neglected the nematodes as material for cytological and embryological research.[2] A search for a suitable material on which to study the effect of heat shock on the division mechanism showed nematode eggs to have many advantages. Since the eggs are about 45 microns in length and relatively transparent many details of pronuclear fusion, spindle formation, aster formation, spindle elongation and cell cleavage may be readily observed with the 4 mm objective. Their potentialities as living material for class use became apparent, and a two-year trial has proved their value.

Any earthworm will serve as a source of nematodes, where they may be found in the body cavity or in the nephidia. A convenient method of starting a culture is to allow bits of the body wall of the earthworm (nephridial region) to decay on 2 per cent. water agar in a Syracuse watch glass. After four days at 18° the nematodes are mature. I maintain subcultures at 18° on 2 per cent. agar, using bits of beef (3 cu mm) as food. The worms are prolific, and great numbers can be cultured with ease. Worms four days old are the best for eggs, since older worms usually have few cleaving eggs (although many older embryos are present) in the uterus. It is best to make subcultures every day so that worms of a proper age will always be available. Temperature control is essential —very few eggs are found if worms are reared at room temperatures of 22° or above.

In order that the cleavage process may be studied by students it is necessary to pick out ten or fifteen mature females from the culture dish, using a binocular and a fine curved needle (a number 12 sewing needle serves admirably). The worms are put in a drop of tap water on a depression slide and torn part with a straight pair of number 12 needles. The eggs are sucked up into a micropipette and are distributed to depression slides containing tap water. They may of course be studied on an ordinary slide, providing the following precautions are taken: (1) that they are not crushed; (2) that they get adequate oxygen; (3) that they do not dry.

The rapidity of development must be considered when class study is contemplated. Fortunately, the entire sequence of events from pronuclear fusion through the second cleavage occurs within a two-hour interval at average temperatures.

Attention is called to the absolute necessity of isolat-

[1] R. Rugh, SCIENCE, 85: 588, 1937.

[2] Beamis and King, *Biol. Bull.*, 73: 99, 1937, are one exception to this generalization for they have recently published the results of ultracentrifugal experiments on Ascaris eggs.

ing pure lines for use in the experimental work, for earthworm nematodes have been shown[2,3,5] to vary extremely in their reproductive behavior. There are bisexual types, parthenogenetic types, hermaphroditic types and merospermic types. Workers will wish to consult the work of Spek[4] and Bělăr[5] both for their subject-matter and for their bibliography.

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A USEFUL METHOD FOR MOUNTING INSECTS

THE most common method employed in making permanent insect mounts for the classroom entails the use of riker mounts. However, the expense involved, the inability to see more than one side of the specimen, the tendency of the cotton to obscure important parts and the necessity for careful protection against insect destruction makes their use of questionable value for small insects. A modification of the usual slide mounting method involving the use of thick celluloid cells for hermetically sealed dry mounts has proved much more satisfactory in this laboratory for insects the size of a honeybee or less.

The cells are prepared by marking off the celluloid (Gardiner Brothers, San Francisco, 30/1000 to 90/-1000 inch) into 22 mm squares with the aid of a sharp knife and breaking it into strips the length of the sheet and 22 mm wide. By using a die and punch (Fig. 1) it is possible to punch holes rapidly in squares

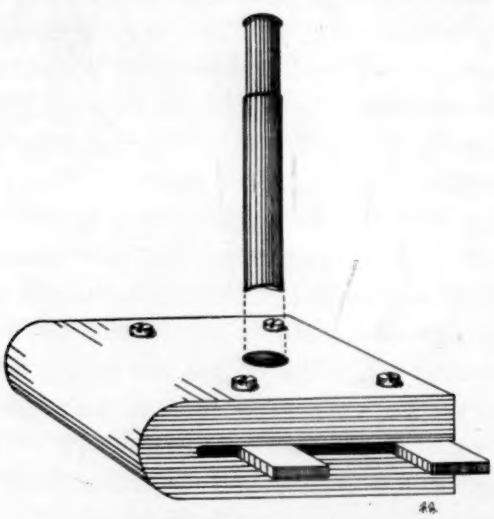

Fig. 1

even of very thick celluloid (90/1000 inch). By the use of various dies or a file it is possible to obtain cells of any desired shape and size to accommodate the insect.

These cells may be fixed to the microscope slide by dipping in butyl acetate and putting in place on the slide or by allowing a drop of butyl acetate or similar solvent to spread by capillary action between the slide and the cell. In a few minutes the cell is firmly fixed to the slide, and the insect may be attached by a very small amount of dissolved celluloid, euparol, and so forth. The mount is completed by covering the cell with a cover glass and allowing a drop of butyl acetate to flow between the slip and the cell. The preparation is ready for use within five minutes and the insects are completely protected until the slide or cover glass is broken.

This method is especially useful for mounting fragile insects such as owl midges, mosquitoes, white flies and other insects bearing delicate scales which might be lost or altered by mounting in other ways. Distortion and shrinkage due to drying seems to be much less in insects mounted in this manner. The cells fixed on slides may be used in three or four minutes as very satisfatory wells for larvae or insects which can not be mounted dry and must be placed in a mounting fluid. Very acceptable life history mounts may be made from celluloid blocks having four holes to accommodate the stages of the insect. These may be mounted dry or in media, depending on the stage (Fig. 2). Larger insects may be mounted in double thickness cells.

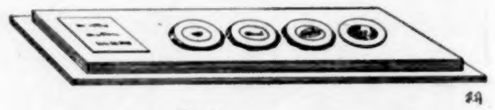

Fig. 2

Slides made in this manner have been used in our laboratory for over two years and have proved very satisfactory. The insects may be viewed from both sides, the mounting is rapid and inexpensive and storage or filing is easily accomplished.

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